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DARWIN *VS.* GALIANI.[1]

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IT was a hundred years ago, in the *salon* of the Grand-Val, after dinner. Here was assembled that fastidious company of wits as well known to us through the letters of Diderot to Mademoiselle Voland as though we, too, had been guests under Holbach's roof. There was Diderot himself, the most German-like of Frenchmen, and Grimm, the most Frenchy of Germans[2]; that peevish Scot, Hoop, and the little Neapolitan abbé, Galiani,[3] in whom playfulness and levity often concealed profound thinking. There, too, were those women whose redoubtable charms are immortalized in Rousseau's "Confessions," as those of Helen in the "Iliad" and "Odyssey."

The fortunate ones of this world had then a good time, especially in France. The trammels of superstition which for seventeen centuries had made slaves of the human race seemed to have been burst asunder. The sun of a cloudless day was illumining and warming the intellectual world; while on the other side of the Atlantic the dawn of popular freedom and human dignity was beginning to appear. Despotism in church and state was tottering before assaults which daily

[1] Translated from the German, by J. Fitzgerald, A. M.

[2] Sainte-Beuve, "Causeries du Lundi," third edition, vol. ii., 1858, p. 203.

[3] Ferdinand Galiani was a native of the province of Abruzzo, Naples, born in 1728. He was no less remarkable for wit than for solid acquirements. Having made a collection of specimens of the volcanic products of Vesuvius he sent them to the Pope in a box thus labeled, "Beatissime pater, fac ut lapides isti panes fiant" (i. e., Most Holy Father, command that these stones be made bread); in answer to which the Pope gave him the canonry of Amalfi, with four hundred ducats per annum. He wrote a treatise on "Money," "Annotations upon Horace," "Dialogues on the Corn Trade," etc. He held several important offices under the Neapolitan Government, and died, greatly esteemed, in 1787.—(Cates's "Biographical Dictionary.")

grew bolder; and Cazotte's prophecy[1] had not as yet cast its ominous shadow over this gay and brilliant *coterie*. Moreover, what was there now that man might not do, after Montgolfier had ascended in the air, and Franklin had subdued the lightning?

The company talked first of the great American citizen; then of the great Frederick, who also was conceded to be somebody; next of Voltaire, with whom Frederick seemed to be fully reconciled. But, with all their worship of Voltaire, and with all their willingness to recognize his services in the cause of enlightenment, there was no getting over the fact that, in truth, he was an incorrigible deist.[2]

"How childish," remarked the host, "is his conception of the universe as a watch from which we may infer the watch-maker! Inasmuch as nothing is certain save the existence of matter, why look for any other causes than the forces of matter? What is there so inconceivable in the idea that an infinity of atoms, acting upon one another from eternity, should assume a definite arrangement, and so form worlds; that when in these worlds light, heat, moisture, and certain elements were present in the right proportions, the phenomenon which we call life should first appear as a germ, and then expand into ever-widening and more varied circles; that in this way the animal mechanism, and, finally, that of man himself, should have come into being?—a well-contrived mechanism, indeed, but not without many an imperfection; endowed with strength and beauty, but also afflicted with many a sad infirmity; capable of enjoying many a pleasure, but also threatened, alas! with still crueler sufferings."

There was general assent, but then were heard coming from a corner the piping tones of Galiani's voice:

"Ladies and gentlemen, for Heaven's sake, no metaphysics to-day! Let us talk of something else. Suppose I tell you of an occurrence I once witnessed on the Marina at Naples. A juggler had set up his booth, around which stood a group of lazzaroni and other folk, myself among them. After performing sundry tricks which have escaped my memory, the fellow offered us a wager that he could every time, with his dice, throw sixes. One or two of the gapers took up the bet. He did, in fact, throw sixes the first time, the second, third, fourth—"

"But, monsignor, this is a farce; either you are joking, or else the dice were cogged."

"Of course they were," Galiani replied. He had, as was his wont, climbed into his arm-chair, on which he squatted with his legs doubled up beneath him; he had his wig poised on his left hand, for the weather was hot, and, with his right, he gesticulated wildly, after the manner of his countrymen. "Of course they were cogged, and that was just the trick. The juggler did not say that he would throw sixes every time

[1] "Œuvres choisies et posthumes de M. de la Harpe," Paris, 1806, tome i., p. 62.

[2] E. du Bois-Reymond, "Voltaire in seiner Beziehung zur Naturwissenschaft," Berlin, 1868, p. 19.

with honest dice. Any man in his senses might have known beforehand that the dice were cogged, and the fellows who found this out only after their money was gone were laughed at heartily. But the point of the story is this: If two dice fall on the same side four times in succession, you, not being lazzaroni, hold it to be impossible that the thing should happen by accident. You conclude, with undoubting certitude, that a hidden cause, designed to produce this effect, has been incorporated in the dice, in the shape of a little lead. But, when you see all around you this universe, with its innumerable suns, planets, and moons, which, poised in vacancy, have for thousands of years been rhythmically traveling in their courses, without ever a collision; when you see on this globe dry land, sea and atmosphere, sunshine and rain, so distributed that myriads of plants and of terrestrial, aquatic, and aërial animals swarm in joyous life; when, for all these creatures, you see the alternation of day and night, of winter and summer, beneficently answering, in all respects, to the requirements of activity and rest, cessation and growth; when, in your own body, you see each particle of its ineffably complicated structure performing exactly the functions which the good of the whole organism demands, while in turn it can itself subsist only in the whole; when, in your own members, your eye, your ear, you see the profoundest science of the mechanician or the optician so far transcended that our friend D'Alembert and the great Euler yonder in St. Petersburg, *e tutti quanti*, appear as fools; when you see this machine—alongside of which your Le Roy's finest watch is, as it were, some coarse piece of mill-gearing, your Vaucanson's most ingenious automaton a wretched toy—perfecting itself by practice, making its own repairs; when you see it even reproduce its own kind, and male and female most charmingly, mother and child most beautifully adapted to each other; when, in the Jardin du Roi, under a thousand animal forms, from the elephant to the shrew-mouse, M. de Buffon shows you as many types of your own organization, each one adapted in its own way for the enjoyment of life and the pursuit of its prey, for defense against its foes, for propagation of its kind, and for care of its young; when you see the bees solving their cell-problem as correctly as the most learned of mathematicians, the spiders bracing their polygons of silken threads, the mole excavating its galleries, the beaver constructing its dams; when, further, in all these instances you see the agreeable combined with the useful, and magnificence, ornament, and grace everywhere lavishly displayed—Flora's children clothed with beauty, the gaudy butterfly flitting about among them, the peacock spreading his tail-feathers; finally, when Mr. Needham shows you, under his microscope, how each drop of vinegar or of paste is alive with creatures as numerous as the worlds you have been able to descry through M. de Cassini's telescope—you confidently say that all this is chance. And yet the spectacle presented to us by nature is the same as though some one were every instant, with an infinite number of dice, to make exactly the

throw which he announces in advance. I judge differently, ladies and gentlemen. I say that nature's dice are cogged, and there, above, is the greatest of jugglers making sport of us."

What reply was, on the spot, made to the abbé we know not. But of the impression which the apologue of the cogged dice made on the encyclopædists we learn something from a passage in the "Système de la Nature," that work which, in the opinion of the young Goethe and his Strasburg associates, was "senile, cimmerian, cadaverous, the very quintessence of senility, offensive to all correct taste, nay, insipid." And yet it is not to be denied that the "Système de la Nature," in most points, very nearly represents the idea of the universe now held by scientific men.

In that work Holbach vainly squirms to escape from the snare in which he had been caught by the Neapolitan. "The molecules of matter," says he, "may be likened to cogged dice, i. e., they always produce certain effects of a determinate kind. Inasmuch as these molecules are in themselves and by their combinations essentially diverse, we may say that they are cogged in infinitely diversified ways. The brain of Homer or of Virgil was nothing but an aggregate of molecules, or if you please of cogged dice—i. e., things so constituted and so elaborated that they must of necessity produce an Iliad or an Æneid."

To say nothing of the fact that Holbach speaks of mental phenomena being produced by material conditions as of a self-evident proposition, nothing could be more awkward than the mode in which he strives to wrest the weapon from the hand of his opponent. By adopting the comparison of the molecules of matter with cogged dice, he unwittingly admits that in nature, just as in a gambler's den, there is trickery; whereas the problem before him was to explain how material particles not directed toward any definite end should nevertheless cooperate to that end.

Here is the knot, here the enormous difficulty, that racks every understanding that would comprehend the universe. Whoever will not surrender all occurrences into the hand of Epicurus's Chance, whoever admits even the veriest tittle of the doctrine of teleology, must perforce accept Paley's disparaged *natural theology*, and this the more inevitably the more clearly and accurately he reasons, the more independently he exercises his judgment. But so weighty and so numerous are the facts which seem to favor teleology; so irresistibly do these facts daily force themselves upon us in common life; so interwoven are final causes with time-honored imaginations of our race instilled into us during childhood, that even minds possessed of considerable powers of abstraction can not in their habitual thoughts refrain from postulating them. A man may, with Lichtenberg, ridicule the teleological explanations offered in earlier times. Be he ever so determined to regard the processes occurring in the animal body simply as effects produced by the mechanical or chemical organs, and so to represent them to others,

nevertheless presently he finds himself speaking of the use, functions, and purposes of the organs.

The possibility, however remote, of banishing out of nature this apparent adaptation to ends (teleology), and of everywhere setting up blind necessity in the place of final causes, is to be regarded as one of the greatest advances ever made in the world of thought, as a step from which will be dated a new epoch in the treatment of these problems. That he has in some measure diminished that torture of the mind which tries to understand the universe, will be Charles Darwin's highest title to fame so long as there exists a philosophic student of nature.

Mr. Darwin's "Origin of Species" undoubtedly found zoölogy, botany, and paleontology in a state bordering on *doctrinaire* lethargy. The knowledge of organic forms was daily increasing in an overwhelming proportion. The sole concernment of scientific men was, as far as possible, to classify the superabundant harvest in the existing systematic frame-work; and where this could not be done, to extend the latter and add to it on this side and on that, as need was. Natural history in its narrower sense, i. e., the study of the modes of life and the instincts of animals, was hardly to be found anywhere save in books for children. As for ascertaining the meaning of the facts gathered up by naturalists, as for any theory of organic beings, such things were hardly so much as thought of. The ancient dogmas of the immutability of species—a conception which, however, no one was able to define—of the infertility of hybrids, of successive acts of creation, of the impossibility of spontaneous generation, of the recent origin of the human race —these dogmas precluded all effort in that direction. The earlier attempts, in our own time brought to light again, of Lamarck and others, at solving this problem with the aid of insufficient data, and in part from the point of view of nature-philosophy, had fallen into oblivion, and long since it was the custom to regard it as irresolvable by natural science. Independent thinkers who would not bow down before the infallibility of the school were solemnly admonished of the error of their ways. For there existed a hidden community, composed for the most part of people who were unconnected with the zoölogical school, but to which many also within the school now profess to have belonged, though at the time they showed no symptom of it: this party already entertained secret doubts concerning the inerrancy of the received dogmas. Johannes Müller himself, who in other respects clung to these dogmas with strict orthodoxy, who as a professor inculcated them on his pupils, and who labored with indefatigable industry in building up the orthodox system, betrayed, on the occasion of his discovery of the development of *Mollusca* in holothurians, heretical tendencies which brought him into no little trouble with the school.

It is a pity that he did not live to witness the catastrophe which only one year after his death overtook this very self-assured school.

The disaster was of a kind never before seen in the history of Science—so long preparing and yet so sudden; so quietly planned and yet so mighty in its effect; a scientific event without a parallel, whether we consider the extent and the importance of the system which was overturned, or the reverberations of the downfall to the uttermost limits of human knowledge. Just as, after the overturning of thrones, the neighboring countries are for a long time all commotion and tumult, whereas in the center of the disturbance itself new institutions are beginning to take root, so in consequence of the Darwinian movement the always unsettled border-land between science and philosophy is yet in a state of violent fermentation, as we see nearly every day from the many-hued bubbles of literary effort which come to the surface. But in the field of calm and earnest science this first turmoil has given way to more quiet contemplation. Already a new generation, which has grown up amid this revolution, is beginning to take the lead with renewed courage. Leaving out of the account a few cross-grained geniuses, whose lamentations we can disregard and pass to the order of the day, it is on all sides admitted that the old ground was untenable, and that in the place of Cuvier's and Agassiz's series of creations must be substituted Mr. Darwin's doctrine of descent.

At the same time the opinion seems to be steadily growing that the evolution of organic nature is to be explained only by the so-called laws of organic structure. The peculiar merit assigned to Darwin is that he has gained the victory for the doctrine of descent. On the other hand, the doctrine of natural selection is regarded, at the best, as an ingenious idea, set forth with much skill, but in point of fact of no value.

This view, in my opinion, calls in question the very best portion of the new advance in science.

Compared, indeed, with the doctrine of the systematic school, as it reigned unquestioned in manuals and lecture-rooms down to the publication of Darwin's work, the doctrine of descent appears in itself to be a great step of progress. None can be more ready to admit this than they who therein see the triumph of their own views, silently entertained; none can more highly esteem than these pre-Darwinian Darwinists the merit of having aided in giving to the doctrine of descent its present commanding position. Still it was not to be expected that these men should feel as much indebted, intellectually, to the doctrine of descent, as others who before were not quite certain whether whales didn't come forth ready made out of nothing; whether each species manufactured at pleasure by the ornithologist or the entomologist was not created in the beginning, and did not enter Noah's ark. If naturalists of this class, though now they accept the doctrine of descent, are nevertheless a little uneasy in their consciences, and wonder at their own audacity, it is, on the other hand, perfectly natural that those older adherents of the doctrine of descent should not rest content with the triumph of their

views, but that they should follow even in his further conquests the great leader who by a single blow won the victory for their side. But the end which he points out to us is high above the doctrine of descent, which doctrine, in so far as it attempts to explain the evolution of organic nature solely by its laws of development, is in fact still of little avail to us.

We must remark, in the first place, that what morphologists denominate laws are not laws at all in the sense of theoretical science. These so-called laws are simply rules deduced from a greater or less number of cases, and like grammatical rules only serve to classify and explain, by a process of vicious circle-reasoning, other facts embraced within the same definition. Even Kepler's laws were but rules of this kind, until Newton deduced them from the universal law of gravitation, and so raised them to the dignity of laws. But now that they are firmly based on the principle of gravitation, the whole doctrine of the movements of the heavenly bodies may be inferred from Kepler's laws with the highest attainable degree of certitude; and our longing to know the causes of things is as fully appeased by this explanation as the nature of the human understanding will permit. We know, with that kind of certitude which we denominate absolute, that, like the planets of our own solar system, so those of unseen suns move in ellipses whose *radii vectores* describe equal areas in equal times, and that the squares of their revolution times are as the cubes of their distances from their suns.

It is very different with the laws of organic structure. If in a Jurassic rock we find a fragment of a rhombic enameled scale, we infer with a very high degree of probability that the fish of whose panoply this scale was a part thousands of years ago had an independently pulsating aorta-peduncle. If on breaking up a shapeless piece of fossil bone we discover a spiral auditory cochlea, we know that the animal of whose skull the fragment was a part was a mammal. It is no small triumph that we dare make such assertions as these. Still, there is not absolute certitude here. Even the most firmly established laws of organic structure possess only a greater or less probability. Absolute characters are in systematics the philosopher's stone. True, in some cases the probability grounded on laws of organic development borders on certitude. That we shall never find a centaur, pegasus, griffin, a configuration like that of an angel or a demon, whether living or fossil, we may affirm with almost the same certainty as that a planet which has never been observed will obey the laws of Kepler. Whether we can with equal certitude affirm that never will a vertebrate be found in which by a transposition of the central nervous system the posterior and the anterior roots of the spinal-cord nerves will have interchanged functions, may perhaps be open to doubt, however improbable such a thing may be. Would the comparative anatomist ever have supposed *a priori* that such a structure could exist as that of the *Pleuronectæ*? Then, in the Invertebrata the uncertainty of the laws of organic structure is

in so much greater as the Invertebrata, with perhaps the exception of the Articulata and the Radiata, are less pronouncedly typical and physiologically less intelligible.

This uncertainty of the laws of organic structure comes from the fact that these laws themselves are purely experimental; containing in themselves no ultimate, logically cogent truth such as we find in physico-mathematical laws. Hence, a departure of nature from these rules implies no contradiction, no impossibility; and if a thing is not impossible, of course it is possible.

Physico-mathematical laws form, as it were, a resting-place on which we may safely step without fearing that it will ever fail under our weight. In the history of development, on the other hand, what has been our experience? Within a short period of time, a very restricted survey of the animal world, a survey guided by chance, made us acquainted with a series of facts that conflict with all our previous knowledge. Discoveries like that of the inverted position of the embryo in certain rodents, of the development in the deer, of alternate generations, of the development of the echinoderms, of the *Entoconcha mirabilis*, of parthenogenesis, of hectocotyly—all these are calculated to put us on our guard against premature generalization in this field. But in fact such anomalies as these are only counterparts of others with which we have long been familiar, as the marsupials, viviparous fishes, etc., which make no impression on us, because already known to science.

Under such circumstances the application of the biogenetic fundamental law to individual cases is very hazardous, even though we admit the principle in a general sense. The inferences which ontogeny, guided by a few scattered paleontological characters, permits us to draw with regard to phylogeny, will never possess more than a very restricted degree of probability. It will ever be open to the individual understanding to take whatever way it chooses amid the confusion of innumerable and complex possibilities, and, excepting a few indisputable points, which, however, were understood long ago, to conceive a great many different modes of development of the organic world as it exists now. As for certain genealogies of our race drawn up in unfettered presumptuousness rather by an artistic imagination than by a scientifically trained mind, they are of about the same value as the pedigrees of Homeric heroes in the eyes of the historical critic. For my part, if I want to read a work of fiction, I can find something better than a "History of Creation."

But this is not the point which concerns us just now. Granting the scheme of descent from the little mass of protoplasm with which life is supposed to have begun, up to man himself, to be clearly made out (which it is not), the fashioning of organic nature will, after all, be as great a riddle as ever, if laws of structure have alone determined its development.

And this, not because molecular mechanics, which produces modifi-

cations of structure, is a sealed book to us, and will for ever remain so. The molecular mechanics of crystallization, of chemical processes, seems, it is true, to be more easily understood than that of cells; still, for the present it is as hidden from us as is the latter, though its unintelligibility is of a different kind. It is the adaptation seen in the development and in the functions of the cell which, even were we acquainted with the descent of all forms, would still leave organic nature a mystery. By laws of structure alone we can not explain adaptation in organic beings. Hence, however complete our doctrine of descent, the ancient riddle which has confronted mankind from the beginning persists with all its original obscurity, unless something else comes to succor us. The sphinx of teleology still threatens unconquered from her crag. What boots it to know the reason why all vertebrates are made up of the self-same homologous parts, if we do not further know what natural cause so transformed these parts as to make them exactly answer to the purposes of each separate species? If, to explain this latter fact, a supernaturalistic intervention is still necessary, then we are yet in about the same old rut. Formerly the question used to be why, in repeated creative acts, Omnipotence always clung to the same models, and at times did only indifferent work? But now we must ask why it should, in advance, have tied its own hands, committing itself to faulty constructions and making it impossible for itself, e. g., to create a vertebrate with six extremities, though in a given case such a plan might be a very serviceable one. Hence we are, on the whole, no better off, and have only altered the form of the problem, without coming nearer to a solution.

In these straits we find in the doctrine of natural selection a measurably acceptable solution. Associated with the laws of structure, it would forthwith enable us to understand why organized beings are so wonderfully adapted to one another and to the world around them; why in themselves they are adapted to these ends, at the same time, however, exhibiting many an inadaptation; why they always stand in groups made up of the self-same parts, as though Nature had not been able to invent something new, while nevertheless each one of these parts is cleverly so transformed as to answer a special purpose in each species. Sexual selection, then, comes in to perfect the weapons, offensive and defensive, of the wooing male animal, and furnishes the answer to the question how animated nature happens to lavish plumage adornment on birds; whereas Maupertuis's theorem of the smallest action precludes any superfluity in inanimate nature. Even the glowing hue of Alpine flowers is accounted for by the attraction which brighter-colored individuals exercise upon the insects scarce in those heights, and necessary for fertilization. And mimicry, a fact brought to light by Mr. Alfred Russel Wallace, to whom we owe an important share in the discovery of the grand principle of natural selection, still further multiplies the conditions under which new forms come into existence and

become fixed. Finally, this principle extends into the field of psychology and throws light even upon the origin of our ethical aspirations. In short, in lieu of final causes we would have in organic nature a most complex but blindly acting mechanism; and the cosmic problem would be reduced to the two enigmas: "What are matter and force?" and, "How can these think?"

The objections to this doctrine of natural selection are essentially three in number:

The first group of its opponents simply question the facts on which the theory is based, namely, the tendency to variation, the transmissibility of varieties, the fecundity of hybrids, the mutability of species; above all, Darwin's very ingenious explanation of the dying out of intermediate forms. These opponents, however, urge but little beyond the arguments on which the doctrine of the old systematic school rested, and which have been shown by Darwin to be untenable. Still, there is one objection which possesses undoubted weight. I myself early called attention to it in my lectures, in which I believe I was the first public expositor of the new doctrine in Germany. The objection was not printed till much later, so far as I know, and then by Prof. A. W. Volkmann. It is this, that the minute variations in which new species are supposed to have their rise can not be of any material advantage to the individual in which they appear. Still, in my opinion, this objection applies only in certain cases, and perhaps only provisionally. In the case of electrical organs, for instance, it still seems to be unanswerable, for we can not assign any possible use for the so-called pseudo-electrical organs. But, as concerns wings, we see in the example of the flying opossum, the flying lemur, and of the flying frog, discovered by Wallace, how difficult it is to say of a rudimentary organ whether it is or is not of advantage to an animal. In short, the question is not whether this or that definite structure, but whether any adapted structure whatever, can be explained in the way pointed out by Mr. Darwin. In many cases of adaptation by mimicry, and of sexual selection, this is admitted by the great majority of naturalists; and this, as we shall see, is for the present enough.

The second group of opponents do not question the general correctness of the principle or the validity of natural selection in certain cases. But they object that the principle does not explain all structures. To suppose that it must, implies a misapprehension. It never was pretended that natural selection could, by itself alone, account for the fashioning of organic nature; laws of organic structure have always been supposed to act simultaneously with it. Mr. Darwin himself has dwelt on this aspect of the problem, but, as was natural, it has no paramount place in his treatise, despite its importance. If I mistake not, in the innumerable essays which have been written on the Darwinian theory, sufficient stress has not been laid on the fact that the laws of organic structure must account for whatever in organisms is either not

adapted or is even dysteleological; while, on the other hand, natural and sexual selection has to account for most of what is adapted or what is present for ornament's sake only, and which is therefore unexplainable by the simple laws of structure. So truly is this the correct view of the case, that in fact the structure of organisms seems everywhere to be a compromise between the requirements of the laws of structure and the effects of natural selection, as we see—to employ an illustration familiar to the physiologist—in the crossing of the air-passage by the digestive passage in lung-breathing vertebrates, an arrangement full of peril to life. On a previous occasion I showed that, in accounting for this state of things, the Darwinian theory coincides, in its results, with the optimism of Leibnitz. Still I am very far from overlooking the difficulties which still remain on this point. One of the most serious of these, in my opinion, is the power of regeneration, as it is called by physiologists, and the nearly allied *vis medicatrix naturæ*, whether as seen in the healing of wounds, in the circumscription and compensation of internal morbid processes, or—at the outermost end of the series—in the reproduction of a complete fresh-water polyp out of each of the two halves into which one such polyp has been divided. This power could not have been acquired through natural selection; and here it seems inevitable for us to recognize laws of structure working toward an end. But have we not a like phenomenon in the restoration of mutilated crystals, a fact observed by Jordan, Lavalle, Pasteur, Sénarmont, Scharff, and others? So, too, the power which organisms possess of perfecting themselves by practice appears to me not to have yet been sufficiently studied with reference to natural selection.

As a third argument against the theory of natural selection—one which is supposed to negative all its claims to consideration—its opponents always urge in the last place that in no single instance has any one ever actually observed adaptive transformation of an organ by inheritance and selection of the fittest forms. What transformations have been thus effected in the past no man can tell, it is objected; and inasmuch as, even in the future, observations and experiments on this subject seem to be impracticable on many accounts, it is claimed that the doctrine of natural selection is not only an unproved hypothesis now, but that it is fated for ever to remain so. Taking their stand upon this ground, and contrasting themselves with the believers in Darwinism, its opponents boast not a little that they are upholding the standard of strict method, which requires us to accept as demonstrated only what is proved by experiment or by mathematical reasoning.

But this again is a mistake. If it is conceded that any one adapted structure can be explained by natural selection, and if therefore this theory is admitted to be legitimately deduced from legitimate premises, then, in order to suppose the operation of natural selection, wherever it is needed to explain phenomena, it is not necessary for us to actually demonstrate such operation in the individual instance. It might be an

exceedingly difficult thing to distinguish, in the performance of a complex machine, the part played by certain forces, as gravity and inertia, everywhere operating according to known laws. Nevertheless, no one would doubt that gravity and inertia do coöperate in the machine, nor should we for a moment hesitate to ascribe to the one or the other of these two forces whatever subordinate effects are only explainable by each respectively. And so in the present case. Natural selection is not, like the supposititious laws of organic structure, an empirical rule which may to-morrow, perhaps, prove nugatory. Neither, indeed, is it, like mathematico-physical laws, an infallible leading principle governing material events. But, as being a proposition deduced by a chain of valid inferences from universally admitted facts, and at the same time a proposition necessary *in se*, natural selection stands midway between a rule and a law, though it comes nearer to the latter. Hence, of the two evolution principles of organic nature, laws of structure and natural selection, the latter is in theory the surer, whatever may be its shortcomings in practice.

Undoubtedly it were much to be desired that we could in the individual instance demonstrate the working of natural selection, and follow the process step by step. But this we can not reasonably expect to do. Between the work of natural selection for one generation and the result after 100,000 generations, there subsists about the same ratio as between differential and integral. How seldom it is that we are able to understand this latter ratio, even though we subject it to calculation! But do we for that reason question the correctness of our integration? The corresponding problem, in the present instance, would be to investigate and ascertain the evolution of a species through an endless line of generations and under diverse external conditions, while at the same time, as has been already stated, unintelligible laws of structure, working either not adaptively or only accidentally so, enter the problem as undetermined constants, or even as undetermined functions. Though this can not be done, it does not follow that we must misapprehend the ratio between the differential and the integral found for us by nature, as though by a calculating-machine.

Thus, then, so far as the validity of the principle in general is concerned, it may be for us a matter of indifference whether or not in the individual instance we can discern and demonstrate the operation of natural selection. As things stand, it must be operative, and the only question is, whether its influence, as regards quantity, is comparable to that of the laws of structure, or whether other more powerful influences obliterate its effects, so that the adaptation prevailing throughout nature would be attributable solely to the action of these laws. In view of this question, the following appears to me to be the proper attitude of the investigator of nature:

That natural selection can perform what we must ascribe to it in

order to explain thereby the adaptation of organic nature, is no more demonstrated than is the contrary proposition. The aim of the theoretical investigator of nature is to understand nature. If this aim is not to be an absurdity, the man of science must presuppose the intelligibility of nature. Final causes in nature are incompatible with nature's intelligibility. Hence, if there is any way of banishing teleology from nature, the man of science is bound to take it. Such a way is found in the theory of natural selection; and hence we must follow in it. Be it that, in holding this theory, we experience the sensations of a man who as his only hope of rescue from drowning clambers on a plank which can only just keep him above the water: when the choice lies between a plank and drowning, the plank has a decided advantage.

Galiani's apologue does not now puzzle us as once it puzzled the encyclopædists. We should have known how to reply to it, for Mr. Darwin has enabled us to see why it is that nature generally, though not always, throws doubles, and that, too, without cogged dice. And, as in our opinion systematics did not attain its true significance and its full value till now, when it no longer deludes itself with its artificial frame-work of classification, so even in physiology we continue to make use of teleology as an aid in discovery, but with the understanding that, the teleology of organs being apparent only, there will also be much that is unteleological, or even antiteleological.

On the other hand, a man is not to be censured who, under the influence of such impressions as we have described, finds it impossible to believe that all nature, the human brain included, was created by the forces of matter out of a chaotic nebulous sphere. What, at the utmost, seems possible when applied to a minute mass of protoplasm, will appear rather hard to believe even to the most uncompromising monist, when he looks at a human blossom, beaming with grace and genius; and yet the difference between such mass of protoplasm and a human being is a difference simply of degree; in fact, the human being was once a mass of protoplasm. In matters of this kind, personal bias, determined by natural constitution, education, and accidental influences, will ever play a great part: Teleology and Vitalism—both in one shape or another as old as mankind—will last as long as the race itself. Hence, let every man take his own course; only, the partisans of Final Causes must not imagine, as they are wont to do, that they offer a better solution of the problem, or any solution at all that is worthy of that name, when they invoke the aid of supernatural conceptions of any sort.

This was well understood by Leibnitz. True, he did indeed suppose that he had discovered a dualistic theory of the universe, but the part he there assigns to final causes proves the correctness of the remark just made. Leibnitz utterly rejected teleology in the material world. Here, for him, reigns mechanical causality, and nothing else. Matter is, according to him, created by God, but at the same time it is so invested once for all with motive force that there is no need of setting

the clock of the universe to make it go right. The sum of matter, like the sum of motive force, remains ever the same. Whatsoever has occurred or ever shall occur in the material world is ideally determinable mathematically. In a word, the material world is a mechanism, only infinitely more ingenious than any mechanism contrived by man, and composed of an infinity of parts inclosed one in the other.

Alongside of this machine of the corporeal world, Leibnitz supposed a spirit world—the world of his monads—whose ideas, from their creation down, keep pace with the changes of the corporeal world and answer to them; but between them and the corporeal world no reciprocal action of cause and effect is possible. On this theory, when we suppose we are working for ends, or that we have sensations produced by external causes, such ideas are preëstablished phantasms of our soul-monad, which is ever presenting to itself exactly the course of things that is passing outside at the same instant, and that seemingly, but not in reality, works through or upon the monad. Once only, miracles apart, has anything been done in the universe for an end, according to Leibnitz, and that was when God created the universe as perfect as he could. How Leibnitz supposed it possible to reconcile his theory with freedom of will, is a question which does not concern us here.

Thus, there was no doubt in Leibnitz's mind that material particles may, in virtue of the forces imparted to them, constitute an apparently teleological universe. Nay, all difference between his and our theory of the material universe vanishes if God created the world infinite ages ago. But even if God created the universe at the finite time $-t$, the course of events necessitated by Leibnitz's theory corresponds perfectly with what it would be in our theory, onward from the instant $-t$. For, inasmuch as Leibnitz looks on the condition of the universe at each instant as a function of time, God could, according to him, create the world in the instant $-t$ only in that condition in which it was at that time, according to our view.

Take away from Leibnitz's theory of the universe the illusory appendage of the monadology, of preëstablished harmony, and of optimism, and the only solid nucleus that remains is his mechanical conception of the material world, and his perception of the impossibility of explaining on supernatural grounds a material fact, or, conversely, of explaining on mechanical grounds a spiritual fact. His having, over and over again, clearly and sharply expressed this perception—indeed, it was this perception that forced him to resort to the hopeless idea of preëstablished harmony—may well be esteemed to be Leibnitz's special service to metaphysics, though he himself, and his followers, hitherto make that claim rather for those more brilliant speculations. Certain it is that matter, as we have to do with matter in physico-mathematical studies, is not all, not the substance of things. But what there is over and above matter is hidden from us; and when we strive to set up before

our mind's eye objectively a spiritual substance, we simply deny the properties of matter as reported to us by our senses, and hence the product of our phantasy proves incapable of causal action and reaction with matter.

How profoundly in error, then, are they who, often in a tone of scientific pharisaism, lament our blindness in trying to account for the universe without final causes which so easily and so beautifully solve all problems, even those of ethics! These people simply show that at bottom they are ignorant of what knowledge means. For us there is no other knowledge save mechanical knowledge, however beggarly a substitute that may be for true knowledge, and consequently there is only one truly scientific form of thought, the physico-mathematical one. Hence there can be no more mischievous illusion than that whereby we are led to believe that we explain the teleology of organic nature by calling to our aid an immaterial intelligence, conceived in our own likeness, and working for ends. It is of no consequence what form we give to this anthropomorphism; whether with the "Timæus" of Plato we postulate as an emanation of Deity in living beings moving ideas, with which never any definite conception has been connected in anybody's mind; whether with other philosophers we suppose an unconscious soul which constructs bodies after the types of their various kinds ever present to itself, which sees through all the enigmas of physics and chemistry, and which is thus far more intelligent than the conscious soul; or, finally, whether with Leibnitz we suppose God to have once for all in the beginning ordered the universe with a view to ends. It is, I repeat, of no consequence under which of these forms one attempts the impossible. So soon as one quits the region of mechanical necessity, he enters the boundless cloud-land of speculation. But it is all to no purpose; for, if the teleological character of nature weaves a crown of thorns for monism, at the same time her occasional antiteleology is anything but a bed of roses for dualism. The appeal to the advantages presented by dualism for the explication of ethical problems is of no avail with one who knows the true state of the case. Must we over again be reminded of the obscurity which Leibnitz vainly labored to remove in his "Theodicy"?

The student of nature in the present day can only assume the attitude of resignation toward the ultimate principles of things. I have in another place shown how the palpable errors of such thinkers as Leibnitz can be explained by the times in which they live. Between Leibnitz and ourselves there is an enormous chasm dug by scientific research, reënforced by observation and experiment, by calculation and induction.

Above all, qualitative research, so called, has on the scientific mind an educating influence, like that of life on character. Being corrected at every step by nature, and constantly reminded of the uncertainty of his judgments and the fallaciousness of his apparently most firm con-

clusions; being sooner or later infallibly punished for every over-hasty opinion, for every act of blind trust in appearances: such is the discipline which accustoms the experimental scientist to be chary about rapid and brilliant conquests; to attack the truth which he fain would discover by gradual approaches; to test it as impartially as though his aim were to prove the contrary; and finally, when he is arrived at a number of perhaps mutually contradictory facts held together by a tissue of still obscure relations, and the whole looking toward sundry possibilities whereof experience alone can decide which is stronger, resignedly to keep that state of things present to his mind as the best it is conceded him to know.

Surely it appears as though mathematical research, too, which proceeds inductively to a greater extent than is commonly supposed, might have a like educating effect. It, too, possesses what is lacking to metaphysical speculation, the sure means of determining whether its judgments are correct or not. But the mathematician draws this determination from himself, and hence his occupation is less adapted than experiment for weakening one's trust in speculation. Hence it is that mankind could for two thousand years busy itself about problems in mathematics, without ever curbing their propensity to speculation; hence, again, Descartes and Leibnitz, two of the greatest mathematicians of the seventeenth century, were also the boldest metaphysicians of the same period.

Hardly two centuries have elapsed since chemists, physicists, and physiologists went to work steadily and systematically, and already we see the fruits of their teaching, as transmitted from generation to generation. In this school the human mind has lost the habit of childish reverence and juvenile enthusiasm, grown up to the discretion of manhood, and learned to comport itself modestly in presence of insoluble enigmas. A new phase of its history is observable, partly in the decay of speculativism, and partly in the style of philosophizing now adopted by the best minds.

The practice acquired by the man of science in the small warfare of the laboratory fits him to deal with the great mystery of the universe. The striving which we observe in Leibnitz, toward constructing by hook or by crook a universe wherein preconceptions inherited from the childhood of the race are blended with the insight of an already well-matured physico-mathematical mind, is a thing so foreign to the man of science that he could no more think of adopting that point of view than of adopting the mythological cosmogony of the Hellenes or of the Brahmans. The complacent assurance with which Leibnitz looks on his scheme as demonstrated, reminds him of similar illusions in the beginning of his own scientific development, for in the domain of mind, too, the biogenetic principle holds good. Knowing well how immovably fixed are the bounds set to man's understand-

ing, he has no longing to transcend them. He sees that his field of research lies between the problems, "What are matter and force?" on the one hand, and "How do matter and force think?" on the other; outside of this field he knows only that he knows nothing, can know nothing, and will know nothing. Standing without vertigo on this mountain-summit of Pyrrhonism, he scorns to people the vacuity round about him with the images of his own phantasy and surveys unappalled the unpitying drift of nature without gods. He is not disheartened at the thought that he stands face to face with eternal enigmas. He does not, like an Empedocles, cast himself into the physical abyss whose secrets he is unable to fathom; nor, like a Faust, into the ethical abyss, although no unworthy trammels restrain him from yielding to its temptations. For he contemns not reason and science, though it be denied him to recognize the first cause of things. Like Lessing, he holds the higher good to consist not in the possession but in the pursuit of truth; and therefore does he find solace and exaltation in *labor* which increases the store of human knowledge; which by healthy effort enhances the powers and the capacities of our race, extends our dominion over nature, ennobles our being by enriching our mind, and beautifies it by multiplying our joys.

From the disheartening conclusion, "*Ignorabimus*," the student of nature recovers as he pronounces the stirring countersign given by the dying Septimius Severus to his legionaries—[1]

"*Laboremus!*"

SCIENTIFIC RELATION OF SOCIOLOGY TO BIOLOGY.

By Professor JOSEPH LE CONTE.

II.

BIOLOGICAL METHODS APPLICABLE TO SOCIOLOGY.—We have thus shown the use in sociology of the ideas and doctrines characteristic of biology. We have shown that they are applicable, but with some limitations and modifications imposed by the presence of a nature higher than the animal. We come now to show the use of biological *methods* in the cultivation of sociology.

The great characteristic method of biology is the method of comparison. The reason is obvious. The phenomena of life are so complex that it is impossible to reduce them to law without simplifying

[1] Jussit deinde signum tribuno dari *Laboremus*, quia Pertinax quando in imperium adscitus est signum dederat *Militemus*.—"Scriptores Hist. August. ab Hadr. ad Numerianum."

them. But the mode of simplification adopted in physics and chemistry, viz., *experiment*, or the arranging of simple artificial conditions, is only to a slight extent applicable to biology. The phenomena are not only complex, but they are so delicately balanced that the introduction of our rude hands in the way of experiment often overthrows the equilibrium, destroys the conditions of biological experiment, viz., life, and thus throws the whole subject into the realm of chemistry and physics. But, fortunately, nature has prepared for us an elaborate series of experiments. We have organisms of every degree of increasing simplicity, from the body of man to the microscopic spherule of almost unorganized protoplasm called a moner. The complex problem of life, as we go down this scale, is made simpler by the successive removal of added complications, until it is finally reduced to its simplest terms, and thus only we begin to understand the essential phenomena—thus only may we find the value of the unknown quantity. It is, therefore, by extensive comparison of organisms in all stages of complication with each other, that the foundations of a scientific biology have been laid. Anatomy has become scientific only through comparative anatomy; physiology through comparative physiology; and, we may add, psychology is now awaiting the development of comparative psychology.

But this general method of comparison is subdivided into three or four sub-methods. Nature has prepared not only one but three or four series, not identical, not mere duplicates of each other, but varied, yet resembling and illustrating each other. The first of these is the natural history or *taxonomic* series. It consists of the whole series of organisms as they now exist, from the complexly structured mammal to the simple unicelled plant or animal. The second of these is the embryonic or *ontogenic* series. It consists of the successive stages of development of one of the higher animals, from the germ-cell to the mature condition. The third is the geological or *phylogenic* series. It consists of the organisms inhabiting the earth in successive epochs, from the Archæan until now. The fourth is the *pathological* series. It consists of all possible variations from the normal type by monstrosity or by disease. Though much less full than either of the others, it must not be neglected by biologists.

It is wholly by extensive comparison in these four series that biology has recently risen to the rank of a true science. In this great work, the chief credit is due to three men, viz., Cuvier, Agassiz, and Darwin; for these three are the great founders of the comparative method. Cuvier laid the foundations of comparative anatomy and physiology, by comparison in the taxonomic series. Agassiz extended the comparison to the ontogenic and phylogenic series, showed the resemblance between the three, and determined and announced all the *formal* laws of evolution of the organic kingdom as now recognized. Darwin has made the bold and in large measure successful attempt to explain these laws by the operation of secondary causes. If Agassiz may be called the Kepler

of the time universe, Darwin may with some though with much less show of reason be called its Newton. I say with *less* show of reason, for the causes of evolution are yet very imperfectly known.

Now, not only are all these methods applicable to the study of sociology, but all the advance in this science which has taken place in recent times has been the result of their application. For how has sociology been advanced, and must continue to be advanced? 1. By the comparison of social organisms, nations, tribes, etc., as they now exist in different portions of the earth and in different grades and kinds of civilization, with each other, in institutions, habits, customs, forms of government, etc. Is not this the taxonomic series? 2. By comparing the different stages of development of the same social organism, from savagism to the highest degree of civilization, and marking the origin, growth, and modification of government, institutions, customs, etc. Is not this the embryonic or ontogenic series? 3. By comparing with each other the successive stages of advance of *all* social organisms of the whole race through the rude stone, the polished stone, the bronze, and the iron conditions. This is M. Comte's historic method; but is it not the geological or phylogenic series? 4. By comparing the same social organism with itself in its normal and abnormal conditions, i. e., in a state of peace, prosperity, social health, and social sanity, with the same in various states of commotion, revolution, anarchy, social fever, and social frenzy. Is not this the pathological series? It is impossible to doubt that these are the true scientific methods of sociology. But they were all first used in biology, and only afterward imported into sociology.

But it will perhaps be objected: "This supposed relation of sociology to biology is but an *analogy* which has not even the merit of being new or recent. It has always been recognized. It is well expressed by the story told by Menenius Agrippa to the mutinous Roman plebeians, in which he showed the absurdity of their conduct by comparing the condition of Rome to a state of war among the members of the body. It is also admirably expressed by St. Paul in his comparison of the church to a well-organized body with different members having different functions. The analogy has always been recognized, but has not borne any special fruit in the advancement of social science, or the betterment of the social condition." To this I answer: Yes, it has always been recognized; but there are different degrees of recognition, and it is *only the higher degrees which bear any scientific fruit.* In this, as in other departments, a recognition of the laws of nature by the *imagination* gives rise to metaphor, simile, poetry, art, and in its highest manifestations is what we call genius. A *dim*, imperfect recognition of the same by the reason constitutes *analogy.* The clear recognition by the reason of the same in all its details, so that the application of appropriate methods becomes possible, constitutes science. Thus sociology, like

all other sciences, has its three stages: 1. The facts of sociology are collected and recorded in chronicle and history. This is descriptive sociology. 2. These facts are reduced more or less successfully to general formal laws. This is philosophical history, or formal sociology. Thus far sociology is built upon its own basis of facts and phenomena; an analogical connection with biology may have been recognized, but not a scientific connection. 3. These formal laws are connected with and explained by the fundamental laws controlling organisms as their cause, and sociology becomes finally a *causal* science. In this, as in all other sciences, this last step is attended with prodigious impulse and steady advance, for it is this last step which connects it with the hierarchy and gives it the assistance of all other sciences. It is this step which has only recently been made, and its effect is already visible.

But it will be again objected that society is already highly organized; how, then, can it be said that the science of social organization is of recent origin? How could the principles of social organization be embodied without a knowledge of those principles? The answer to this is quite plain, and brings to view an additional resemblance between society and other lower forms of organization. As the organic body passes from lower to higher and still higher forms without any will or consciousness or knowledge of the process on the part of the organism itself—or, still better, because more closely analogous to the social organism, as the organic kingdom, regarded as an organism, throughout all geological times developed into higher conditions without any intention on the part of the many individuals of which it is composed, but only as the natural result of each seeking its own ends in the struggle for life—even so society advances to more and more highly organized conditions without any intention on the part of the individual members, much less any knowledge of the principles of social organization, but purely as the natural result of the struggle for life, and each member seeking his own immediate ends. In both cases it is natural law working out its legitimate result. In both cases it is God (for natural law is the mode of Divine activity) working to a given end without the conscious coöperation of individuals. But there is this wide difference: In the latter case, if the development continues, there inevitably comes a time when man turns about and reflects upon what he has unconsciously or at least intuitively done; there eventually comes a time when he consciously coöperates with God or nature, and strives by the use of reason and science to modify and improve the social organism.

Or, regarding it from a slightly different point of view, the social organism is a work of art, the noblest of all arts. Now *art* always *precedes science*, and not the reverse, as many seem to suppose. It is fortunate that it is so, or emergence from barbarism would be impossible. The art of walking is acquired in great perfection before the principles of equilibrium involved are understood. Handspikes and pulleys and

screws were used before the principles of the lever and inclined plane involved in their use were understood. The art of music was carried to exquisite perfection before the scientific principles of harmony were known. Pottery, agriculture, and many other useful arts were not only practiced, but carried to a high degree of perfection, before the corresponding sciences were born. The capacity for art is inherited from the animal kingdom. Science is born of humanity, and is therefore distinctively human. Art is born of instinct or intuition; science of the conscious reason. Art is the result, at first, of the empirical method; science always of the rational method. I repeat, then, art precedes, leads upward to the comprehension of science; but science, when sufficiently perfect, turns again and perfects art. Art is the earthly mother of science; but the celestial daughter, when mature, turns again and blesses and helps her mother. Even so is it with human society. The art of government and of social organization precedes the corresponding science, and is its necessary condition; but when sufficiently perfect, sociology will undertake to modify and better the social organization, and guide the social development.

But I said, art is inherited from the animal kingdom; science is born of humanity. For this reason art is of itself limited; science is unlimited, for it is connected only with the highest nature of man, the spiritual and eternal. Art may be carried to a high degree of perfection, but unless assisted by science quickly culminates and declines, or else becomes petrified and immutable; but if its principles be understood, if it becomes allied with science, if it passes under the dominion of the self-conscious reason, it then becomes indefinitely progressive. So also that highest art, the art of government and social organization, may reach, unassisted by science, a high degree of perfection; but if it be simply an art it quickly culminates and declines, or else becomes petrified and immutable, as we see in the Chinese and Japanese. As in the organic kingdom each organic form is specialized to the greatest extent and then becomes immutable and generally perishes; as the tree of life branches, and each branch grows its several way—flowers, fruits, and dies—even so each isolated nation, or branch of the social tree, pushes its growth it knows not whither, but as far as possible in its direction, then flowers, fruits, and dies. But if the scientific principles of sociology be once understood, if science or self-conscious reason guide the social development, there can no longer be any limit to its progress. But observe: this indefinite progress is due wholly to the introduction of other principles than those derived from purely animal nature; it violates the perfect analogy to material organisms. In fact science, which determines and guides that indefinite progress, is itself born only of that higher nature.

But again: all who have reflected much on the relation of science to art can not fail to have observed that while a *mature* science guides art with certainty, and continues to perfect it without limit, yet there

is a certain stage in the development of science when its influence may be even disastrous. In the passage from art to science there is a certain stage when the presumptuous application of an *imperfect* science interferes with the truer and better results of a perfect empiricism, and art is thereby hurt. This is true especially of the more complex arts and sciences. Thus the principles of science must be held in subordination to an enlightened empiricism in such arts as medicine, agriculture, etc. Science can not yet undertake to guide these arts with confidence. So is it also, and in a much greater degree, in the case of human society; for here we have the most difficult art, and the most complex and imperfect science. It must be yet a very long time before the science of sociology can presume to guide the course of social progress. Premature interference with the results of an enlightened empiricism can do nothing but harm.

I have now covered the ground which is implied in the title of this article. I have shown the close connection, both in doctrine and method, between social and organic sciences—a connection similar to that which exists between organic science and that immediately below it in the hierarchy. I have shown that sociology is one of that hierarchy, and the highest. I have shown that the cultivation of this science requires acquaintance with all other and simpler sciences, but especially biology. My task would seem to be done. But I would do violence to my feelings and convictions, and would be liable to serious misconstruction, if I stopped here. What I have thus far said gives but an imperfect idea of the comprehensiveness and complexity of social science. I have yet shown but one side of this complex subject, although the side which is most familiar to my thoughts, and, I believe, also the best developed. It is necessary at least to glance at the other side. I have developed *one* of the foundations or basic connections of sociology; but there is another, as I now proceed to show.

All along, in the course of this discussion, I have from time to time shown that there are certain limitations to the application of the doctrines and methods of biology to sociology, and that in every case such limitation is the result of the introduction of some new principle characteristic of *humanity* as distinguished from *animality*, of *reason* as distinguished from *instinct*, of *spirit* as distinguished from *matter*. This is precisely what, even from a purely scientific point of view, we ought to expect, and is in fact necessary. For in the scientific hierarchy each science, in addition to the forces and phenomena of the lower sciences, deals with a new force and a new group of phenomena, and therefore with new doctrines and new methods. In going up the scale of sciences we rise successively to a higher and higher plane of activity. On the plane of dead matter only physical and chemical forces operate, and only physical and chemical phenomena occur. On the plane of living matter, in addition to the preceding, we have also vital forces and

phenomena. On the plane of sentient existence we have, in addition, nerve force and phenomena. On the plane of rational and moral existence we have, in addition to all the preceding, also rational and moral forces and their corresponding phenomena. With every rise to a higher plane we have also, as has already been shown, new doctrines and new characteristic methods. Shall we not, then, on this highest plane also, viz., on the plane of rational and moral existence, the plane of humanity, shall we not have here also new characteristic methods and doctrines connected with this new and higher form of force? It is evident that we must. All the doctrines and methods which I have developed are imported from biology. I have said nothing of characteristic methods and doctrines of sociology. Comte clearly saw the necessity, in accordance with the principles of a scientific hierarchy, of a characteristic method, and he thought he had found it in what he calls the historic method. But Comte's historic method, as we have already shown, is nothing but comparison in the phylogenic series—a method which is imported from geology. There must be characteristic methods and doctrines in this highest science also. What are they? Man is certainly something more than an animal. What is that something more? The answer to these two questions is the same. The characteristic doctrines and methods of sociology are evidently connected with man's higher rational and moral nature—with his distinctive humanity. But the science of this side of our nature is yet so imperfectly developed, our knowledge of these higher phenomena is yet so imperfectly reduced to law, that these characteristic doctrines and methods are not clearly recognized and distinctly formulated. In a word, our knowledge here is not yet scientific; the department has not yet even a distinctive name. For want of a better we shall call it psychology, although it really includes much more than usually goes under that name. But when (if ever) this department of knowledge shall take on a scientific form, then it also must become another basis, another fundamental science, on which must rest sociology. And what sociology is now waiting for, more than for anything else, is the scientific development of this second basis.

Thus then sociology, unlike other sciences, and because of man's twofold nature, rests not on one only but on *two* more fundamental sciences. The basis which I have developed is the material basis. This is all that the materialists admit. If a pure material philosophy were sufficient, this is all the basis which sociology requires. The fact that it is not sufficient, the fact that another basis is required, is demonstrative against a pure material philosophy. According to a pure material philosophy science is a *straight* shaft rising ever until it pierces heaven. But, on the contrary, if we watch its progress closely, we perceive that it indeed rises straight enough *at first;* but as it approaches the plane of humanity it begins to lean and curve to one side, until it inevitably falls over, unless it be supported on that side also. That support which

it must receive is, or should be, the rising and arching shaft of psychology. But, alas! this shaft is yet too imperfectly built to support effectively, and hence the present unsteady condition of the whole fabric of science so far as it relates to man.

Thus, then, there are two fundamental sciences upon which sociology must rest. These are on the material side biology and on the spiritual side psychology. But sociology, like all other sciences, must first rise on its own basis of observed and collated facts and phenomena. Therefore we may describe the process of the building of a scientific sociology as follows: First, social facts, or building materials, are gathered in chronicles and detailed histories: this is descriptive sociology. Then these facts are collated and reduced to laws, the materials are chiseled and fitted and cemented into a rising column: this is formal sociology, or philosophic history and political economy. Then the column arches and connects in one direction with the more fundamental science of biology, and in another direction with that of psychology, and thus becomes causal or true scientific sociology. On this triune arch (not double arch, for the columns stand in triangle) rests the broad triply-supported platform of social science, and from this must hereafter rise the beautiful shaft of increasing social knowledge.

On this triply-supported platform there are *three regions* from which spring respectively three shafts, distinct yet united to form one. The social organism is composed of three subordinate organisms; the social body is composed of *three fundamental coördinate corporations*, connected each most closely with one of the supporting columns. These are—1. The political organization; 2. The moral and religious organization; and, 3. The industrial organization: or the *state*, the *church*, and the *guild*. The first is connected directly with the *history* column; the second with the *psychology* column; and the third with the *scientific* column. They may be compared (though the comparison may be considered fanciful) to the three great regions of the organic body, viz., the *head*, the *thorax*, and the *abdomen*, or rather to the three great coordinate systems of the animal organism, viz., the nervous system, the blood and respiratory system, and the digestive system; the first controlling and directing, the second warming and vivifying, the third furnishing aliment. They correspond also to the three great divisions of the psyche, viz., the intellect, the affections, the will: the first directing and controlling; the second giving motive-stimulus, energy; the third, active and executive. They correspond finally to the three subordinate and coördinate courses of a perfect human culture insisted upon in my article "On Liberal Education,"[1] viz.: 1. The *Language-Art* course, commencing with language, ancient and modern, passing upward through literature, art, history, philosophical history, and thus connecting with sociology through the political organization or the state; 2. The *Philosophic* course, commencing with logic and passing

[1] "Southern Presbyterian Review," 1859.

upward through mental philosophy, moral philosophy, and so connecting again with sociology through the religious organization, the church; 3. The *Scientific* course, commencing with mathematics and passing up through the hierarchy of science, as already given, and so connecting again with sociology through the industrial organization, the guild.

I have made only three fundamental corporations of the social organism. Friedrich Schlegel, the celebrated writer on philosophy of history, in a series of articles entitled "Characteristics of the Age," makes five essential corporations rising one above the other in the following order, viz., the family, the guild, the state, the school, the church. But the least reflection, I think, will show that the family and the school belong to a different order from the other three, being *subordinate* and preparatory to them, not *coördinate* with them. The former are *internal, elaborative;* the latter external, visible, public, final results. I am sure that the more we reflect upon this subject, the more we will be convinced that there are only three fundamental and strictly coördinate corporations.

We have seen that it is the guild which is most directly and closely connected with the scientific column, and with our material nature. In accordance with this fact we find that it is this sub-organism which is by far the most perfectly organized. It is in this that we see most perfectly carried out the law of differentiation and specialization of social functions and mutual dependence of parts; and the strongest tendency to identification of the individual life with the social function. In other words, it is precisely here, as we should expect, that we find the nearest approach to the ideal of material organization. In accordance with the same fact we also find that the corresponding department of social science, viz., political economy, is that which is by far the most perfectly developed.

There are three mistakes made by thinkers on the subject of sociology, all founded on a too limited view of the structure of the social organism, each consisting of an attempt to absorb the whole organism into one of the fundamental corporations—to regard the great field of sociology as connected with only one of the supporting columns mentioned above. Lawyers, politicians, statesmen, and indeed people generally, regard sociology as most closely connected with the history column, and would make the state paramount. The state is for them the social organism. Theologians and moralists, on the other hand, would make the church paramount in importance if not absolutely absorbing the others, and sociology as most closely connected with the psychology column. The modern materialist would make the guild the paramount corporation, and sociology as most closely connected through biology with the scientific column. The political philosopher is apt, therefore, to cling only to empirical laws and so-called practical methods, unaware of or denying the connection of sociology with any more fundamental departments of science, and especially its connection with

biology. The psychologist and the theologian are apt to ignore too much the material basis of sociology, the organic laws which through our material nature impress themselves upon the structure and development of society; while the materialist and the political economist are apt to overlook or belittle the importance of the other essential corporations, especially the church, and make sociology nothing more than the highest of the material sciences. But no steady, safe progress in social organization can be made unless we fully recognize the coördinate value of these three, nor in the science of sociology unless we approach the subject from these three sides.

THE CRYSTALLIZATION OF GOLD, SILVER, AND OTHER METALS.

By THOMAS J. GREGAN.

THERE are few chemical experiments so well known as the growth of the "lead-tree" and "silver-tree." These carry our minds back to the times of the alchemists, who called the first "Arbor Saturni," and the second "Arbor Dianæ," and they may be looked upon as the type of a large number of phenomena in which the salt of one metal in solution is decomposed by some other metal.

My assistant, Dr. Hand, and myself have lately been experimenting on these replacements, the metallic crystals which are thus produced, and the forces that act through the liquid. Our more special attention has been given to the mutual action of copper and nitrate of silver. If these two substances be brought into contact by the intervention of water, there grows on the red metal what may be called "trees," and, though the analogy between the crystals and the plants is a very superficial one, still the resemblances of external form are sufficiently striking, and a nomenclature drawn from the garden seems the most expressive.

A microscopic view of the growth of these silver crystals round a piece of copper is a truly beautiful sight; a blue glass underneath increases the effect, but they are best seen when they reflect a strong light thrown upon them. They may also be thrown upon a screen as opaque objects, but the beauty and luster of their surfaces are in this way lost. The crystals of silver thus produced differ both in color and form according to the strength of the solution. If it be very weak—say one per cent.—the copper is fringed with black bushes of the metal, which in growing change color to white without any alteration of crystalline form that can be detected by a powerful microscope. A stronger solution gives white crystals from the commencement, which frequently

assume the appearance of fern-leaves; while the growth from a still stronger liquid reminds us rather of a furze-bush. If the nitrate of silver amount to fifteen per cent. or thereabouts, there occurs a steady advance of brilliantly white moss ; and, if the solution be saturated, or nearly so—say forty per cent.—this moss is very sturdy, often ending in solid crystalline knobs, or stretching out into the liquid as an arborescent fringe.

In all these cases, however, when the solution in front of the growing crystals has been somewhat exhausted, certain prominent or well-circumstanced crystals seem to monopolize the power, and to push forward through the remaining portions of the liquid. This raises beautiful branches which assume a variety of graceful forms, which it is hopeless to attempt to portray by diagrams, but the subjoined figures give some of the more characteristic outlines greatly magnified. The weak solutions produce feathery crystals, as somewhat in Fig. 1, consisting of

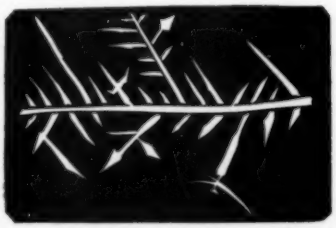


FIG. 1.

FIG. 2.

a straight central stem from which grow on either side crystalline rays that terminate in a sharp point, and frequently become themselves the center stem of a similar crystalline structure.

In the outlying growth of a moderately strong solution the apparent regularity of the crystalline form is lost ; the main stem is built of a confused mass of hexagonal plates, while the side branches are an agglomeration of minute pointed crystals turning in every direction, and producing such jagged outlines as in Fig 2.

FIG. 3.

FIG. 4.

In still stronger solutions the branches lose every appearance of straightness, and they are built of hexagonal plates so studded with

crystalline specks that the whole has the rounded appearance as in Fig. 3. The arborescent crystals that succeed the fringes, from a saturated solution, are smaller in their foliage than the last, and end in small spherical or botryoidal knobs.

Besides these various forms, there occur all kinds of crystalline combinations, as for instance the spray sketched in Fig. 4, when the long and rough branches have terminated each in a large hexagonal plate and the flowing past of a weak solution has afterward caused the growth of delicate fern-leaves. Often, also, a large expansion will take place in every direction, though joined to the parent stem by an almost invisible thread; or, from the point of a long crystal there will branch out to right and left crescent-shaped structures, a process the commencement of which is seen in one of the side rays of Fig. 1. The last traces of silver will frequently give rise to delicate crystalline filaments wandering over the surface of the glass, as in Fig. 5.

Fig. 5.

If a piece of zinc be placed in a solution of neutral terchloride of

Fig. 6.

gold, containing about nine per cent. of salt, there is an immediate outgrowth of black gold, which speedily changes to an advancing mass of yellow or perhaps lilac metal of lichen-like forms, from which proceed beautiful fringes of yellow or black, ending generally in such

arborescent forms as are represented in Fig. 6. As these branches push into the yellow liquid, it becomes colorless even in advance of their points, and it frequently happens that yellow crystals of some salt shoot out in front of the crystallizing metal, which follows them and builds up its advancing fronds at their expense. This is shown in the figure. The gold will generally shoot its yellow branches rapidly round the margin of the drop. Such a running branch has been seen to stop on coming in contact with a loose piece of gold, which immediately in its turn becomes active, and commences to sprout on its farther side. Copper salts give round nodules, which have no crystalline appearance when deposited from moderately weak solutions, but a very strong solution of the chloride—about forty per cent.—yields with zinc, first a thick, black growth, then arborescent fringes of red metal, terminating in crystals of very appreciable size.

The fringes referred to in the case of these three metals are still more characteristically developed by bismuth. When a solution of the terchloride of bismuth acts on zinc there is an immediate outgrowth of black fringes, as in Fig. 7, where they are seen on an illuminated field. As they advance, these become more arborescent, and as the crystalline character becomes more developed they change from black to gray. Sometimes bismuth presents itself in botryoidal masses, but the tendency to form the fringes is very strong.

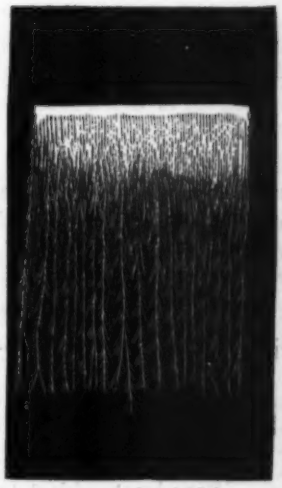


Fig. 7.

Chloride of antimony with zinc also gives black fringes. Lead salts yield crystals resembling those of silver, but leaves of irregular, hexagonal plates prevail, and frequently become of large size.

A solution of acetate of thallium, of twenty per cent. of salt, quickly gives a beautiful forest of thorny crystals. Sulphate of cadmium gives rise to a small, leaf-like growth on zinc; but a strong solution of the chloride produces an appearance of sticks covered with small spines or knobs. The new metal indium is thrown down upon zinc in the form of thick, white crystals. The deposition is promoted by touching the zinc with a piece of iron.

Tin gives beautiful results. If zinc be placed in a solution of stannous chloride it is quickly surrounded with a growth of prolonged octohedra, and as these advance into the liquid it is easy to observe that the additions of new metal commence at the apex, and that the wave of chemical change proceeds down the lateral edge, occupying some seconds of time in depositing the new layer of material. Frequently,

also, there is a luxuriant growth of large, flat leaflets, or symmetrical structures resembling fern-leaves, but with the fronds arranged at right angles, or combinations of these with octohedra, as shown in Fig. 8. These fern-leaves often begin of a dull-gray color, but, as they advance, suddenly change to a brilliant white. The particular form of these crystalline growths depends, therefore, primarily on the specific character of the metal, but this is greatly modified by the strength of the solution.

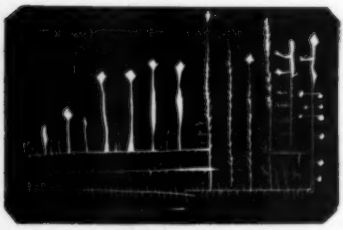


FIG. 8.

The forms assumed by native metals resemble those produced by the process of substitution. In some cases, indeed, it seems almost certain that the deposition of these minerals was effected in the same way—as, for instance, the silver on the native copper of the Lake Superior district.

Gold is frequently found in cubes more or less rolled, but the leaf-gold from Transylvania bears a striking likeness to the crystals that form in our laboratory experiments.

Silver is often found native as twisted hairs or wires of metal—a form that never occurs in the decomposition of its nitrate by copper, but which can be artificially produced in another way. There has been noticed a singular tendency in old silver ornaments and coins to become crystalline and friable. I have an ancient fibula from the island of Cyprus, supposed to be at least fifteen hundred years old, which, through the greater portion of its substance, presents a fracture something like that of cast iron, and its specific gravity has been reduced in round numbers from ten to nine.

It contains a little copper. This property of certain metals, or their alloys, to change in condition and in volume, is worthy the attention of those whose duty it is to make our standards. Experiments should be instituted for the purpose of learning what metals or combinations of metals are least subject to this secular change.

These metallic crystals are Nature's first attempt at building. The material is the simplest possible—in fact, what chemists look upon as elementary. But how is the building carried on? What are the tools employed? Where are the bearers of burdens that bring and prepare the pieces and lay them together according to the plan of the Great Architect? We must imagine what is taking place in the transparent solution. The silver, of course, existed at first in combination with the nitric element, and for every particle of silver deposited on the growing tree an equivalent particle of copper is dissolved from the surface of the plate. The nitric element never ceases to be in combination with a metal, but is transferred from the one metal to the other. On the "polarization theory" the positive and negative elements of

the salt constantly change places and enter into fresh combination, one consequence of which would be a gradual passage of the nitric element from the growing silver to the copper plate. This actually takes place, and there is a diminution of the salt at the ends of the silver branches, giving rise to an upward current and a condensation of nitrate of copper against the copper plate, which gives rise to a downward current. These two currents are seen in every reaction of this nature. In the case of silver and copper, however, it has been proved that the crowding of the salt toward the copper plate is more rapid than would follow in the usual "polarization theory." The instrument used in determining this point was a divided cell in which two plates, one of silver and one of copper, connected with a wire, are each immersed in a solution of its own nitrate, contained in each division of the cell, and separated merely by parchment paper. The crystals of silver deposited on the silver plate in this experiment are very brilliant. There are indications of the liquid being put into a special condition by the presence of two metals which touch one another. Zinc alone is incapable of decomposing pure water, but if copper or platinum be deposited on the zinc in such manner that the water can have free access to the junction of the two metals, a decomposition is effected; oxide of zinc is formed and hydrogen gas is evolved. At ordinary temperature the bubbles of gas rise slowly through the liquid, but, if the whole be placed in a flask and heated, pure hydrogen is given off in large quantity. We have also found that iron or lead similarly brought into intimate union with a more electro-negative metal, and well washed, will decompose in pure water.

As might be expected, the action of magnesium on water may be greatly enhanced by this method; and a pretty and instructive experiment may be made by placing a coil of magnesium in pure water at the ordinary temperature, when there will be scarcely any visible effect, and, then adding a solution of sulphate of copper, the magnesium is instantly covered with a growth of the other metals, and at the same time the liquid seems to boil with the rapid evolution of the hydrogen-bubbles from the decomposed water.

When, however, the force of the two metals in contact has to traverse a layer of water, the resistence offered by the fluid prevents its decomposition. This must also be an important element in the decomposition of a metallic salt dissolved in water—and, in fact, we have found that the addition of some neutral salt, such as nitrate of potassium, increases the action, apparently by diminishing the resistance of the liquid. If, too, we increase the quantity of the dissolved salt, we get more than a proportional increase of deposited metal. Thus, in an experiment made with different strengths of nitrate of silver, the following results were obtained in ten minutes, all the circumstances being the same except the strength of the solution:

1	per cent.	solution,	0259	copper.
2	"	"	078	"
4	"	"	224	"

In fact, it has been found that, in solutions not exceeding five per cent., twice the amount of nitrate of silver dissolved in water gives three times the amount of chemical action ; and this is true with other metals also in weak solution. It may be that this is not the precise expression of a physical law, but it agrees at least closely with the results of experiment.

The power arising from this action of two metals on a binary liquid may be carried to a distance and produce similar decompositions there. This is ordinary electrolysis. Metals have often been crystallized from their solutions in this way, and I have seen excellent preparations of crystalline silver, gold, tin, copper, platinum, etc., by using poles of the same metal as that intended to be deposited upon them. The forms thus obtained are precisely analogous to those produced by the simple immersion of one metal into a soluble salt of another, and illustrate still further the essential unity of the force that originates the two classes of phenomena.

HERBERT SPENCER BEFORE THE ENGLISH COPYRIGHT COMMISSION.[1]

II.

QUESTION (*Chairman*). I will ask you if you have any explanations you wish to offer on any point connected with the evidence which you gave on the last occasion ?

Answer. Yes ; I have to rectify some misapprehensions. From the restatement made by Mr. Farrar, it would appear that, in discussing the question of profits from republication of one of my works, I said I had "found that no other publisher would undertake the work without an additional profit of ten per cent.," which implies that I had endeavored to obtain another publisher. My meaning was, that I ascertained that any other publisher who thought of issuing a rival edition would expect to make a profit of ten per cent. beyond the ten per cent. commission for doing the business. Further, I have to remark that the case I took as illustrating the improbability that I should obtain any considerable compensation from increased sales under the royalty system was the case of one of my works only, the "Principles of Psychology," and in respect of this I may admit that there would be little danger of a rival

[1] March 20, 1877: Lord John Manners, M. P., in the chair. Present, Sir Henry T. Holland, Sir Louis Mallet, Dr. William Smith, Anthony Trollope; J. Leybourn Goddard, Esq., Secretary.

edition. But it is not so with others of my works—with the work on "Education," now in its fourth thousand; with "First Principles," now in its fourth thousand; and especially with the just-issued first volume of the "Principles of Sociology." These are now sufficiently in demand, and, especially the last, sufficiently popular in manner and matter, to make rival editions quite probable.

Q. Now, with respect to the stereotype plates, would they not enable you to exclude the rival editions of which you speak?

A. I think not. In the first place, the assumption that other publishers would be deterred from issuing rival editions by my stereotype plates, implies that other publishers would know I had them. I do not see how other publishers are to know it, until after I had myself printed new editions—even English publishers—and it is out of the question that colonial publishers should know it. Hence, therefore, the fact of my having stereotype plates would not prevent such rival editions. Consequently these rival editions, making their appearance unawares, would compete with my existing stock, printed in a comparatively expensive style, and would oblige me either to sacrifice that stock or to lower the price to one far less remunerative. Then, subsequently, there would not be the supposed ability to compete so advantageously with editions published by others. An edition to be sold at a cheap rate must not be in large type, well spaced, and with ample margins, but must be in small type, and much matter put into the page. Hence the existing stereotype plates, adapted for printing only books in a superior style, could not be used to print cheap books: the quantity of paper and the cost of printing would be much larger items than to one who arranged the matter fitly for a cheap edition.

Q. Then we are to gather that you do not think that from any such cheap edition you would derive a profit from the royalty compensating you for your loss?

A. Nothing like compensating. Although the sales of these more readable books I have instanced might be considerably increased, the increase could not be anything like as great as would be required to produce the return I now have. Even supposing the price of the rival edition were the same, which of course it would not be, the ten per cent. royalty would bring in the same amount, only supposing four times the number were sold that I sell now; and as, by the hypothesis, the price of the volume, to get any such larger sale, must be much lower, the royalty would bring in so much the less. If, say, "First Principles" were issued at half the present price, eight thousand would have to be sold instead of one thousand, to bring in by royalty the present returns. Such an increase of the sale would be out of the question; even one half of it would be improbable; so that certainly one half of my returns would be lost.

Q. Have you any other personal experience that you wish to bring before the commission to show that such a modification of the copy-

right law as you have been discussing would be disadvantageous to literature of the graver kind?

A. I think I have. "First Principles" was published in 1862, and in the course of some years the doctrine it contains underwent, in my mind, a considerable further development, and I found it needful to reorganize the book. I spent five months in doing this; canceled a large number of the stereotype plates; and was thus at considerable cost of time and money. As I have already pointed out, the work being now in its fourth thousand, has had a degree of success such that there might, under the proposed arrangement, very possibly have been a rival edition at the time I proposed to make these alterations. Had there been such a rival edition, this cost of reorganization to me would have been more serious even than it was; since the difference between the original and the improved edition, adequately known only to those who bought the improved edition, would not have prevented the sale of the rival edition; and the sale of the improved edition would have greatly diminished. In any case the errors of the first edition would have been more widely spread; and, in the absence of ability to bear considerable loss, it would have been needful to let them go and become permanent. A kindred tendency to the arrest of improvements would occur with all scientific books and all books of the higher kind, treating of subjects in a state of growth.

Q. With the object of rendering useful books as accessible as possible to the public, do you think that those engaged in their production and distribution should be restrained from making what might be called undue profits?

A. In answer to the first part of the question I hope to say something presently, showing that the advantage of increased accessibility of books is by no means unqualified; since greater accessibility may be a mischief, if it tells in favor of worthless books instead of valuable books. But, passing this for the present, I would comment on the proposition, which I perceive has been made before the commission, that it is desirable to secure for books "the cheapest possible price consistent with a fair profit to those concerned." I here venture to draw a parallel. What is now thought so desirable respecting books was in old times thought desirable respecting food—"the cheapest possible price consistent with a fair profit to those concerned." And to secure this all-essential advantage, more peremptory, indeed, than that now to be secured, there were regulations of various kinds extending through centuries—alike in England and on the Continent—forbidding of exports, removing of middlemen, punishing of forestallers. But I need hardly recall the fact that all these attempts to interfere with the ordinary course of trade failed, and after doing much mischief were abolished. The attempt to secure cheap books by legislative arrangements seems to me nothing less than a return to the long-abandoned system of trade regulations; and is allied to the fixing of rates of interest, of prices,

of wages. In the past it was the greediness of money-lenders that had to be checked, or, as in France for many generations, the greediness of hotel-keepers; and now it appears to be the greediness of book-producers that needs checking. I do not see, however, any reason for believing that, regulations made by law to secure cheap bread for the body having failed, there is likelihood of success for regulations aiming to secure cheap bread for the mind.

Q. Then do we understand you to mean that no analogy furnished by past experience in commercial affairs can be held to imply that the proposed royalty plan would succeed?

A. I think that all the facts are against it. I find it stated in the evidence lately given that there has not been raised "an insuperable objection in point of principle" to the plan of a royalty. If no such objection in point of principle has been raised, I think one may be raised; the objection, namely, that it is distinctly opposed to the principles of free trade. One of the aims of the plan, as expressed in the words of the same witness, is the "preservation of a fair profit to the author." Now, on the face of it, it seems to me that any proposal to secure fair profits by legislation is entirely at variance with free-trade principles, which imply that profits are to be determined by the ordinary course of business. But, further, I would point out that, if it is competent for the legislature to say what is a "fair profit to the author," I do not see why it is not competent for the legislature to say what is a fair profit to the publisher: indeed, I may say that it is not only as competent but much more competent. I take it to be impossible for the legislature to fix with anything like equity the profit of authors, if profit is to bear any relation to either skill or labor, as it should do; inasmuch as one author puts into a page of his book ten times as much skill as another, and, in other cases, ten times as much labor as another. Hence, therefore, if they are to be paid at the same percentage on the price, there is no proportion in that case secured between the value of the labor and what they receive. Similarly, if we consider the numbers sold, the royalty which might afford ample return to an author who sold a popular book in large numbers would afford little return to an author who produced a grave book selling in small numbers. Obviously, then, it is extremely difficult, and in fact impossible, for the legislature to fix an equitable royalty; but it is by no means so difficult for the legislature to fix an equitable rate of profit for the publisher. The function of the publisher is a comparatively mechanical and uniform function: the same practically for all books, the same for all publishers, and hence is a thing very much easier to estimate in respect of the proportion; and in fact we have the evidence that it can be fixed with something like fairness, inasmuch as publishers themselves voluntarily accept a ten per cent. commission. Hence, I say, not only does the carrying out of the principle imply that if, in pursuit of alleged public advantage, the profit of the author should be

fixed, then also should the profit of the publisher be fixed, but that it is much easier to do the last than to do the first. If so, then, it is competent for the legislature to go a step further. If there is to be a Government officer to issue royalty stamps, there may as well be a Government officer to whom a publisher shall take his printers' bills, and who adding to these the trade allowances, authors' ten per cent royalty, and publishers' ten per cent. commission, shall tell him at what price he may advertise the book. This is the logical issue of the plan; and this is not free trade.

Q. (*Sir H. Holland*). You will hardly contend that the system of royalty is less in accord with free trade than the existing system of monopoly; you will not carry it so far as that, will you?

A. I do not admit the propriety of the word "monopoly."

Q. Without using the word "monopoly," let me say, than the present system of copyright for a certain term of years?

A. I regard that as just as much coming within the limits of free trade as I hold the possession, or monopoly, of any other kind of property to be consistent with free trade. There are people who call the capitalist a monopolist: many working-men do that. I do not think he is rightly so called; and similarly if it is alleged that the author's claim to the product of his brain-work is a monopoly, I do not admit it to be a monopoly. I regard both the term "free trade" as applied to the unrestrained issue of rival editions and the term "monopoly" as applied to the author's copyright as question-begging terms.

Q. Without saying what opinion I hold upon the point, and avoiding the use of the words "monopoly" and "free trade," I wish to know whether you think it most consistent with the doctrines of political economy that every person should be able, upon payment, to publish a particular book, or that only one person should have it in his power to do so for a certain time?

A. Every person is allowed and perfectly free to publish a book on any subject. An author has no monopoly of a subject. An author writes a novel; another man may write a novel. An author writes a book on geology; another man may write a book on geology. He no more monopolizes the subject than any trader who buys raw material and shapes it into an article of trade is a monopolist. There is more raw material which another man may buy. The only thing that the author claims is, that part of the value of the article which has been given to it by his shaping process; which is what any artisan does. The way in which this position of authors is spoken of as "monopoly" reminds me of the doctrine of Proudhon—"Property is robbery." You may give a stigma to a thing by attaching to it a name not in the least appropriate.

Q. (*Mr. Trollope*). I understand your objection to a system of royalties to be this, that no possible *quota* that could be fixed would be a just payment for all works?

A. That is one objection. There is no possibility of fixing one that would apply to all works, inasmuch as the thing paid for is an extremely variable thing, more variable than in almost any other occupation.

Q. I put that question to another witness before you, but I am afraid failed to make him understand me. I am therefore glad to have the answer from you, in order that we may show (I think you will agree with me) that no special royalty specified by act of Parliament could be just to poetry, and to the drama, and to fiction, and to science, and to history at the same time?

A. Quite so. I think it is obvious, when it is put clearly, that it can not be; and that is an all-essential objection.

Q. (*Sir H. Holland*). Nor would it in your opinion be desirable that the question of determining what amount of royalty is proper in each case should be vested in some registrar or some single person?

A. It would make the matter still worse. It would be bad to vest it anywhere, but especially bad to vest it in any single official.

Q. (*Chairman*). Are we to assume that you think the plan of a royalty to be at variance with the established principles of the science of political economy?

A. I think quite at variance with the principles of political economy. The proposal is to benefit the consumer of books by cheapening books. A measure effecting this will either change, or will not change, the returns of those engaged in producing books. That it will change them may be taken as certain: the chances are infinity to one against such a system leaving the returns as they are. What will the change be? Either to increase or decrease those returns. Is it said that by this regulation the returns to producers of books will be increased, and that they only require forcing to issue cheaper editions, to reap greater profit themselves, at the same time that they benefit the public? Then the proposition is that book-producers and distributors do not understand their business, but require to be instructed by the state how to carry it on more advantageously. Few will, I think, deliberately assert this. There is, then, the other alternative: the returns will be decreased. At whose expense decreased—printers', authors', or publishers'? Not at the expense of the printers: competition keeps down their profits at the normal level. Scarcely at the cost of the authors; for abundant evidence has shown that, on the average, authors' profits are extremely small. Were there no other motive for authorship than money-getting, there would be very few authors. Clearly, then, the reduction of returns is to be at the cost of the publisher. The assumption is that, for some reason or other, the publishing business, unlike any other business, needs its returns regulated by law. Thinking, apparently, of prosperous publishers only, and forgetting that there are many who make but moderate incomes and very many who fail, and thinking only of books which sell largely, while forgetting that very many books bring no profits and still more entail loss, it is assumed that the publishing

business, notwithstanding the competition among publishers, is abnormally profitable. This seems to me a remarkable assumption. Embarking in the business of publishing, like embarking in any other business, is determined partly by the relative attractiveness of the occupation and partly by the promised returns of capital. There is no reason to think that the occupation of publishing differs widely from other occupations in attractiveness; and hence we must say that, competing for recruits with many other businesses, it must on the average offer a like return on capital. Were it found that the average return on capital in publishing was larger than in other businesses, there would immediately be more publishers, and competition would lower the returns. If, then, we must infer that, taking the returns of all publishers on the average of books, their profits are not higher than those of other businesses, what would be the effect of such a measure as that proposed, if, as anticipated, it lowered publishers' returns? Simply that it would drive away a certain amount of capital out of the publishing business into more remunerative businesses. Competition among publishers would decrease; and, as competition decreased, their profits would begin to rise again, until, by and by, after a sufficient amount of perturbation and bankruptcy, there would be a return to the ordinary rates of profit on capital, and the proposed benefit to the public at the cost of publishers would disappear.

Q. Then, with a view to the permanent cheapening of books, we may gather that your opinion is that it would not be effected in the way suggested?

A. I think not. The natural cheapening of books is beneficial; the artificial cheapening is mischievous.

Q. May I ask you to explain what you mean by contrasting the natural and the artificial cheapening of books?

A. By natural cheapening I mean that lowering of prices which follows increase of demand. I see no reason, *a priori*, for supposing that publishers differ from other traders in their readiness to cater for a larger public, if they see their way to making a profit by so doing; and, *a posteriori*, there is abundant proof that they do this. The various series of cheap books, bringing down even the whole of Shakespeare to a shilling, and all Byron to a shilling, and each of Scott's novels to sixpence, sufficiently prove that prices will be lowered in the publishing trade if the market is adequately extensive, just as in any other trade. If it be said that in this case authors have not to be paid, I would simply refer to such a series as that of Mr. Bohn, who, notwithstanding the payments to translators and others, published numerous valuable books at low rates. Moreover, we have conclusive evidence that with the works of still living authors the same thing happens, when the market becomes sufficiently large to make a low price profitable. Witness not only the cheap editions of many modern novels, but the cheap editions even of Mr. Carlyle's works and Mr. Mill's works. Deductively and induc-

tively, then, we may say that there is a natural cheapening of books, going as far as trade-profits allow; as there is a natural cheapening of other things. Conversely, I mean by artificial cheapening, that kind which is anticipated from the measure proposed; for it is expected by means of this measure to make publishers issue books at lower rates than they otherwise do. And this is essentially a proposal to make them publish at a relative loss. If, as already argued, the average rates of publishers' profits are not above those of ordinary business-profits, these measures for lowering their prices must either drive them out of the business or be inoperative. To put the point briefly—if there is an obvious profit to be obtained, publishers will lower their prices of their own accord; and the proposed competitive system will not make profits obvious where they were not so before.

Q. But if there was free competition on the payment of the author's royalty, might it not be that another publisher would be led to issue a cheap edition when the original publisher would not?

A. I see no reason to think this. The assumption appears to be that everybody but author and original publisher can see the advantage of a cheap edition, but that author and original publisher are blind. Contrariwise, it seems to me that the original producers of the book are those best enabled to say when a cheap edition will answer. The original producers of the book know all the data—number sold, cost, return, etc.; and can judge of the probable demand. Another publisher is in the dark, and it does not seem a reasonable proposition that the publisher who is in the dark can best estimate the remunerativeness of a cheap edition. If it is hoped that, being in the dark, he may rashly venture, and the public may so profit, then the hope is that he may be tempted into a losing business. But the public can not profit in the long run by losing businesses.

Q. (*Sir H. Holland*). Take the "Life of Lord Macaulay"; you know that Tauchnitz has published a cheap edition in four volumes—a very neat edition, good paper and good print. Is it not possible that if this system of royalty is introduced, without considering whether the author would lose by it, a cheap edition like that would be put upon the market at once, and would pay the publisher?

A. It is possible that it would be done earlier than it is now done. I take it that the normal course of things is that, first of all, the dear edition should be published and have its sale, and supply its market, and that then, when that sale has flagged, there should come the aim to supply a wider market by publishing a cheap edition.

Q. You are aware that one of the advantages which the advocates of this royalty system most strongly dwell upon is that under the present system the great mass of the reading public are not able to purchase the books; those who have the advantage of circulating libraries can get them and read them, but poorer persons can neither purchase nor read them, whereas under the other system an edition like Tauch-

nitz's would be at once put out, and it is contended that this, though it might be a loss to the author, would be a benefit to the public?

A. Then I take it that the proposal really amounts to this: that whereas, at present, the poorer class of readers are inconvenienced by having to wait for a cheap edition a certain number of years, they shall, by this arrangement, be advantaged by having a cheap edition forthwith; which is to say that people with smaller amounts of money shall have no disadvantages from their smaller amounts of money. It is communistic practically—it is simply equalizing the advantages of wealth and poverty.

Q. (*Chairman*). Then we may assume that in your opinion the royalty system would not operate in cheapening books in the long run?

A. I think that in the first place, supposing it should act in the manner intended, by producing rival editions, it would act in cheapening just that class of books which it would be a mischief to cheapen. I have already intimated, in a previous reply, that the alleged advantage of cheapening books is to be taken with a qualification; inasmuch as there is a cheapening which is beneficial and a cheapening which is injurious. And I have got, I think, pretty clear evidence that the class of books cheapened would be a class which it is undesirable to cheapen. Being one of the committee of the London Library, I have some facilities for obtaining evidence with regard to the circulation of various classes of books; and I have got the librarian to draw me up what he entitles—"Recorded circulation of the following books during the three years following their introduction into the London Library." Here, in the first place, is a book of science—Lyell's "Principles of Geology"; that went out twenty-eight times. Here, on the other hand, is a sensational book—Dixon's "Spiritual Wives"; that went out one hundred and twenty times. Here, again, is a highly instructive book—Maine's "Ancient Law"; that went out twenty-nine times. Here is a book of tittle-tattle about old times—"Her Majesty's Tower"; that went out one hundred and twenty-seven times. Here, again, is another book of valuable inquiry—Lecky's "European Morals"; that went out twenty-three times. Here is a book of gossip—Crabb Robinson's "Diary"; that went out one hundred and fifty-four times. Lecky's "History of Rationalism" went out thirteen times; Greville's "Memoirs" went out one hundred and sixteen times. Herschel's "Astronomy" went out twenty-five times; Jesse's "George III." went out sixty-seven times. I have added together these contrasted results, and the grave, instructive books, taken altogether, number one hundred and eighteen issues, while the sensational and gossiping books number five hundred and eighty-four issues; that is to say, more than five times the number of issues. Now, the London Library is, among circulating libraries at least, the one which is of all the highest in respect of the quality of its readers: it is the library of the *élite* of London. If, then, we see that there go out to these readers five times as many of these books

which minister to the craving for excitement, and are really dissipating books, as there go out the grave, serious, instructive books, we may judge what will be the proportion of demand for such books in the public at large. Now let us ask what a publisher will do in face of these facts. He knows what these demands are, and he has to choose what books he will reprint. A publisher who has laid himself out for rival editions is comparatively unlikely to choose one of the really valuable books, which needs more circulating. I will not say he will never do it. He will do it sometimes; but he will be far more likely to choose one of these books appealing to a numerous public, and of which a cheap edition will sell largely. Hence, therefore, the obvious result will be to multiply these books of an inferior kind. Now, already that class of books is detrimentally large: already books that are bad in art, bad in tone, bad in substance, come pouring out from the press in such torrents as to very much submerge the really instructive books; and this measure would have the effect of making that torrent still greater, and of still more submerging the really instructive books. Therefore, I hold that, if the stimulus to rival editions acted as it is expected to act, the result would be to multiply the mischievous books.

Q. (*Mr. Trollope*). Do you not think that, in making the parallel that you have there made, you have failed to consider the mental capacities of readers?

A. I was about, in answering the next question, to deal indirectly with that; pointing out that while there is a certain determining of the quality of reading by the mental capacity, there is a certain range within which you may minister more or you may minister less. There are people who, if they are tempted, will spend all their time on light literature, and if they are less tempted will devote some of their time to grave literature. Already the graver books, the instructive books, those that really need circulating, are impeded very much by this enormous solicitation from the multitude of books of a gossipy, sensational kind. People have but a certain amount of time and a certain amount of money to spend upon books. Hence what is taken of time and money for uninstructive books is time and money taken away from the instructive; and I contend that, if there were a diminution in the quantity of the books of this sensational kind published, there would be a larger reading of the really instructive books; and that, conversely, the multiplication of this class of lighter books would tend to diminish the reading of instructive books. I am now speaking, not, of course, of the higher amusing books, because there are many that are works of value, but of the lower novels, Miss Braddon's and others such.

Q. Do you think that a man coming home, say, from his eight or ten hours' labor in court day after day is in a condition to read Lyell's "Geology" as men read one of Miss Braddon's novels?

A. We are speaking of some ordinary man. No, not an ordinary man, certainly.

Q. Have we not to deal with literature for ordinary men?

A. For both ordinary and extraordinary men; the whole public.

Q. Are not the ordinary men very much the more numerous?

A. Certainly.

Q. Is it not, therefore, necessary to provide some kind of literature, as good as you can, but such that the ordinary mind can receive and can turn into some profit, together with the normal work of life?

A. I am not calling into question in the least the desirableness of a large supply of literature of an enlivening and amusing and pleasant kind, as well as a large supply of graver literature. My remarks point to the literature that is neither instructive nor æsthetic in the higher sense, but which is bad in art, bad in tone, worthless in matter. There is a large quantity of that literature, and that literature I take to be the one which will be most fostered by the proposed measures. I do not in the least reprobate the reading of lighter works if they are good in quality. I refer to the class of works which I regard as not good in quality.

Q. But do not you think you must leave that to settle itself on those principles of free trade which you have just enunciated so clearly?

A. Certainly; I am objecting to a policy which would tend to encourage the one and not encourage the other.

Q. (*Sir H. Holland*). The subscribers to the London Library are, as you say, the *élite* of readers?

A. Yes.

Q. And is not that the reason why there is this difference as to the reading of good and bad books taken out from that library; is it not attributable to the fact that these people have probably bought and have in their own houses the good books, but that they want to look through these other books, and therefore get them from the library?

A. There may be a qualification of that kind; but inasmuch as a very large proportion of the readers of the London Library are ladies, and those who come for lighter literature, I do not think it at all probable that they would have bought Lecky or Maine, or any books of that kind.

Q. I ask the question because I rather think that you will find a very curious difference from that which you have been stating if you go to the Manchester and Liverpool free libraries. You will find there that the workingmen take out largely Macaulay's "History of England" and that class of books.

A. Well, whatever qualifications may be made in this estimate, or the inferences from this estimate, I do not think they can touch the general proposition that books of this kind which in the London Library circulate most largely, are books of the kind which circulate most largely among the general public, and books of the kind which a publisher of rival editions would choose. That is my point.

Q. But might not that very evil to which you refer be met by im-

proving the taste of the majority of the poorer readers, by enabling them to get at once cheap editions of good books?

A. The question is, which are the cheap editions that will be issued. I contend that they are the cheap editions of these books of a dissipating kind; and that the main effect will be to increase the dissipation.

Q. You do not think that the earlier publication of a cheap edition would raise the tone of readers?

A. I do not see that it would do so, unless it could be shown that that would tell upon the graver and more instructive books. My next answer, I think, will be an answer to that.

Q. If you improve the tone of the readers, of course it does tell upon the graver books for those who have time to read the graver books; but there is a large class of readers who have not that time?

A. Yes.

Q. (*Chairman*). Referring to the illustrations which you have just given of works which you would denominate as worthless, or comparatively valueless, did I hear among them historical memoirs and journals?

A. Crabb Robinson's "Diary," for instance; I call that a book of gossip, which anybody may read and be none the better for it.

Q. The question I should like to ask is, are you not of opinion that books of that sort are extremely valuable to the intending historian of the epoch to which they refer?

A. It may be that there are in them materials for him. I have not read the "Greville Memoirs" myself, and I have no intention of reading it; but my impression is that the great mass of it is an appeal to the love of gossip and scandal, and that it is a book which, if not read at all, would leave persons just as well off or better.

Q. Take Lord Hervey's "Memoirs," in the reign of George II.; if you had the privilege of reading that book you would probably say it was an extremely sensational book, but, knowing the position which Lord Hervey occupied in the court and family of George II., I presume we may take for granted that the extraordinary facts which he relates are facts; and if so they would form the basis of a great deal of truthful history, which would be written of that reign: would not that be so?

A. It might be so, no doubt.

Q. Then we understand you to mean that in your opinion the royalty system would not cheapen works that you would describe as valuable?

A. I think, on the average of cases, quite the contrary. I believe the system would raise the prices of the graver books. Ask what a publisher will say to himself when about to publish a book of that kind, of which he forms a good opinion: "I have had a high estimate given of this book. The man is a man to be trusted; the book possibly will be a success. Still my experiences of grave books generally are such that I know the chances are rather against its succeeding. If it should

be a success, and if I had ten years now to sell the edition, I might print one thousand; but, under this arrangement, a grave book not selling one thousand in three years, or anything like it, it will never do for me to print one thousand. Should it be much talked about by the end of three years, there might be a rival edition, and my stock would be left on my hands. Hence, now that there is this very short time in which I can sell the book, I must print a smaller number—say five hundred. But if I print five hundred, and expect to get back outlay and a profit on that small number, I must charge more than I should do if I printed one thousand and had time to sell them. Therefore the price must be raised." In the case of a book which did turn out a success, it might eventually happen that there would be a cheap edition issued, and that that raised price would not be permanent; but this argument of the publisher with himself would lead him to raise the price, not only of that book, but of the other grave books which he published, all of which would stand in the same position of possibly being successes, but not probably; and of these, the great mass, the nine out of ten that did not succeed, the price would remain higher—would never be lowered. There would not only be that reason for raising the price: there would be a further one. If a man in the wholesale book-trade, who puts down his name for a certain number of copies, knows that a cheaper edition will possibly come out by and by, the result will be that he will take a smaller number of copies than he would otherwise do. At the beginning he may take his twenty-five or thirteen, as the case may be; but as the end of the three years is approaching he will say: "No, I will not take a large number; I must take two or three." Then, still further, the reader himself will be under the same bias. He will say: "Well, this book is one I ought to have: I hear it highly spoken of, but it is probable that there will be by and by a cheap edition; I will wait till the end of the three years." That is to say, both wholesale dealers and readers would earlier stop their purchases, thinking there might be a cheap edition; and that would further tend to diminish the number printed and to raise the price.

Q. (*Sir H. Holland*). Might it not be that the publisher, instead of entering into those calculations that you have pointed out, would consider, knowing that other editions may appear: "What is the cheapest form in which I can print this book? What can I afford to give the author consistently with bringing out the cheapest possible book, so that I may be secure against any other publisher bringing out a cheaper edition?"

A. It would be a very reasonable argument, if he knew which, out of these various books of the graver kind, was going to succeed; but since nine out of ten do not succeed—do not succeed, at least, to the extent of getting to a second edition—do not succeed, therefore, so far as to make it at all likely that there would be a rival edition, and that a cheap edition would pay, he will never argue so; inasmuch as he

would in that case be printing, of the nine books that would not succeed sufficiently, a larger edition than he would ever sell. He must begin in all these cases of doubtful grave books by printing small editions.

Q. Where an author brings a book to a publisher, the first question the publisher asks himself is, of course, this: "Is this book likely to take?" and then, if he thinks it will take, he has to consider further, in what degree will it take? Will it have a large sale or limited sale? Because, in each case the book may be a success, though in a different degree. Then, if it is competent for any other publisher to publish an edition, it may be assumed that such edition would be a cheap one; and, therefore, has not the original publisher this further question to put to himself: "The book, I think, will take; but, looking to the chances of a cheaper edition, I must see what compensation I can give to the author, publishing this book as cheaply as possible, so that I may not be underbid hereafter"?

A. But I think that the experiences of publishers show that it does not answer their purpose to run the risk of cheap editions with the great mass of graver books; inasmuch as nine out of ten of them do not pay their expenses—and do not pay their expenses, not because of the high price, but because they do not get into vogue at all. The publisher would argue, "It will never do to print cheap editions of all these ten, because one out of the number will succeed."

Q. Of course he does not do so now, because there is not any possibility of another publisher underbidding him by a cheap edition; but I am assuming a case where any publisher, on payment of a royalty, can publish a cheap edition; then the original publisher would have to consider, "How cheaply can I publish this edition so that I may not be underbid by another publisher?"

A. That, I say, would altogether depend upon the experience of the publishers as to what was, in the average of cases, the sale of a new book. In most instances the sale of a new grave book is very small—not sufficient to pay the expenses; and I think the publisher would make a great mistake if, in the case of such a book, he counted upon getting a large sale at once by a low price. The other argument would, it seems to me, be the one he would use. In fact, I not only think so, but I find my publishers think so.

Q. (*Chairman*). Do you wish to instance any particular case in which you believe that a fixed royalty, such as we have heard about, would have hindered the diffusion of a book of permanent value?

A. Yes, I have an extremely striking, and, I think, wholly conclusive, instance of the fatal effects—the extensive fatal effects—that would have resulted had there been any such system existing as that proposed. I refer to the "International Scientific Series." I happen to know all about the initiation of that. It was set on foot by an American friend of mine, Professor Youmans, who came over here for the purpose. I

aided him, and know the difficulties that were to be contended with, and a good deal concerning the negotiations. The purpose was to have a series of books written by the best men of the time, in all the various sciences, which should treat of certain small divisions of the sciences that are in states of rapid growth—giving to the public, in popular form, the highest and latest results; and it was proposed, as a means of achieving this end, that there should be an international arrangement, which should secure to authors certain portions of profits coming from translations, as well as profits from originals at home, and the hope was that some publisher might be obtained who would remunerate these authors of the highest type at good rates, so as to induce them to contribute volumes to the series. Well, this attempt, after much trouble, succeeded. A number of the leading scientific men of England, France, and Germany, were induced to coöperate. A publisher was found, or rather publishers here and elsewhere, to enter into the desired arrangements; and an English publisher was found who offered such terms to authors in England as led men in the first rank (and I may mention Professor Huxley, and Professor Tyndall, and Professor Bain, and Professor Balfour Stewart, and a great number of others) to promise to write volumes. These men, I know, were reluctant, as busy men, with their many avocations, and their incomes to get for their families, would naturally be, and were induced to enter into the scheme only on its being made manifest to them that they would reap good profits. The English publisher offered a twenty per cent. commission on the retail price, paid down on first publication, and for every subsequent edition paid six months after date; and there were certain smaller percentages to come from abroad. Now, the English publisher proposed to give those terms, knowing that it would be impossible for him to get back his outlay unless he had a number of years in which to do it. He had to stereotype, he had to pay at once these sums to authors, and he had to publish the books at a cheap rate; for, by the way, I ought to have said that part of the plan was that these books should be sold at low prices: I may instance a volume of four hundred and twenty pages for five shillings. These terms would, I take it, have been absolutely out of the question had there been such an arrangement as that under which the publisher, instead of having many years to recoup himself, would have had rival editions to compete with in the space of three years. I do not, however, put that as an opinion. I have taken the precaution to obtain from Mr. King, the publisher, a definite answer on this point. This is the paragraph of his letter which is specially relevant: "Authors can have no difficulty in proving that this" (meaning the system which I told him was proposed) "would be most unjust to them, a confiscation, in fact, of their property; but I, from a publisher's point of view, should like to declare that the terms on which my firm have undertaken the 'International Scientific Series' would be impossible on such a limitation." Now here, then, we have a series of highly valuable books, I

think of the kind specially to be encouraged, amounting to between twenty and thirty already published, and potentially to a much larger number, which would not have existed at all had there been in force the arrangement proposed; inasmuch as the publisher affirms that he would not have offered such terms, and I can testify that, in the absence of terms as tempting as those, authors would not have agreed to coöperate.

Q. (*Sir H. Holland*). Was Mr. King made aware that there would be a limited time within which each volume would be protected?

A. Yes, three years. He did not count upon anything like adequate return in that time. He says, "We are a long way off profit as yet on the series" (I think it is nearly five years since it commenced), "although I am convinced that ultimately we and the authors, too, will be well satisfied."

Q. That would raise the question which I wanted to put, whether in a case like that it would have been possible to publish a cheaper edition than the one now published?

A. Yes, in the absence of the author's twenty per cent.

Q. In the case which you have brought to our notice, may we assume that the cheapest form of edition was published consistently with fair profit to the author and publisher?

A. I think, certainly, with anything like a tolerable mode of getting up. Of course, you may bring down a thing to rubbishing type and straw paper; but I was speaking of a presentable book. They are very cheap for presentable books.

Q. That, perhaps, would be one of the evils arising from a system of royalty, that you would get extremely bad and incorrect editions published of a book, even in the first instance?

A. Very likely.

Q. Because it would be the publisher's object, if that system were thoroughly established, to publish such an edition that another publisher could not underbid him at the end of the three years; that would be, would it not, the general object of the publisher?

A. Yes.

Q. In this case I understand you to say that he could not, consistently with fair profits to the author and publisher, and consistently with its being a properly printed work, without which a work of that kind would be of very little value, have published a cheaper edition?

A. He could not.

Q. And yet he would not have been able to publish such an edition if he had to run the risk of being underbid?

A. Certainly not. He says, "I confess my idea, in proposing such terms as those of the 'International Scientific Series,' looked forward to a yearly increasing interest in scientific literature, and an ever-enlarging circle of readers able to appreciate books of a high class." So he was looking for a distant effect.

Q. I am anxious, as Mr. King is not here, to get your own opinion upon that point: do you concur in his views?

A. Yes, certainly.

Q. (*Chairman*). Have you any further reasons for thinking that measures of the kind which we have been discussing, taken in the interest of cheapening books, might end in doing the reverse?

A. I think there is another way in which there would be a general operation of this system of rival editions, which would have, indirectly, the effect of raising the prices all round; namely, the waste of stock. It would inevitably happen that every publisher of an original edition would, from time to time, have a rival edition make its appearance before his edition was sold. In that case his remnant of an edition, gotten up in a relatively expensive style, would either have to be not sold at all or sold at a sacrifice. Further, it would happen from time to time that two publishers, unknown to one another, would issue rival editions, both of which would not be demanded; there would therefore be a waste of stock. Evidently the system of competing with one another in the dark would continually lead to production in excess of demand. What would be the result? If there is an increased percentage of waste stock, that has somehow to be paid for, if business is to be carried on at all. And as we know that tradesmen have to raise their average prices to cover their bad debts, so, if publishers find an increase of bad stock, they must raise their prices to cover the loss on bad stock.

Q. (*Mr. Trollope*). Would not the ordinary laws of trade correct such an evil?

A. This interference with the laws of trade would entail an abnormal production of waste stock. Under the present system a publisher does not publish a cheap edition till the other is gone; but under the proposed system, with cheap copies perhaps sent from the colonies, there must be waste stock.

Q. When the system had been in operation for a time, do you not consider that that evil would correct itself by the ordinary laws of trade? We are aware that at first the disruption of an existing state of things will create much confusion, and such evil as you have described; but are you not of opinion that this would rectify itself after a time?

A. I do not see how it could rectify itself, if the system of rival editions continued, and operated in the way that it is expected to do. But as I have already indicated by certain hypothetical remarks, I do not think it would continue and operate in that way. I say, however, that if rival editions were issued by men not knowing each other's doings, there must from time to time occur in the business of each publisher loss of stock.

Q. (*Chairman*). From the answer to the last question that has been put by Mr. Trollope, I gather it to be your opinion that the ar-

rangement would be practically inoperative, so far as the anticipated competition was concerned?

A. I think that, after a period of perturbation, a period of fighting and general disaster in the publishing business, there would arise a tacit understanding among publishing houses, which would, in a large degree, defeat the purpose of the measure; and I say this on the strength of definite facts furnished by trade-practices in America. These facts I have from the before-named American friend, Professor Youmans, with whom from time to time, when over here, I have had to discuss the probability of pirated editions of my own books in America. My books in America are published by a large house there, the Appletons; and they deal with me very fairly—pay me as well as any American authors are paid. I have gathered from Professor Youmans that the danger of the issue of rival editions of my books in America is very small; because there exists among the American publishing houses the understanding that, when one house brings out an English book, other houses will not interfere: the mere circumstance of having been the first to seize upon a book is held to give a priority, such as is tacitly regarded as a monopoly. That condition of things has been established through a process of fighting; for when it did at first happen that American houses brought out rival editions of the same English book, or one edition, rather, after another, that, of course, was a declaration of war between the two houses, and immediately there was retaliation, and it ended in a fight. The house attacked revenged itself by issuing, perhaps, a still cheaper edition, or by doing the like thing with some work subsequently published by the aggressing house; and after bleeding one another in this way for a length of time there resulted a treaty of peace, and a gradual establishment of this understanding, that they would respect each other's priorities. If that is what happened in America, when the only claim that a publisher had to the exclusive publication of a book was the claim established by prior seizing of it, and prior printing, much more will it happen here in England, among publishers who have paid for their books, or who have entered into arrangements with authors for half profits, or what not. Having established certain equitable claims to these books, they will very much more decidedly fight any houses that interfere with them, by issuing rival editions. If the men who have ill-founded claims fight, still more will the men who have well-founded claims fight. Hence there would occur among the English publishers, when this system came into operation, a period of warfare lasting, probably, for some years, and ending in a peace based on the understanding that any publisher who had brought out a book would be regarded as having an exclusive claim to it, and would not be interfered with. The fear of retaliation would prevent the issue of the rival editions.

Q. (*Sir Henry Holland*). And therefore would prevent the publication by a rival publisher of a cheaper edition?

A. Yes.

Q. (*Chairman*). Then on the grounds that you have explained, you think the system would become before long wholly inoperative?

A. Not wholly inoperative, I think: inoperative for good, not inoperative for evil. In the course of this early phase to be passed through, in which houses issued rival editions against each other and got into this state of warfare, it would happen that the weaker would go to the wall: the smaller publishers would not be able to stand in the fight with the larger publishers, and they would tend to fail. And further, although treaties of peace would be eventually reached between the more powerful publishers, who would be afraid of each other, and dare not issue rival editions of each other's books, there would be no such feeling on the part of large publishers toward small publishers. If a small publisher happened to issue a successful book, a larger publisher would have no fear in issuing a rival edition of that. Hence, therefore, the tendency would be for the small publishers to be ruined from having their successful books taken away from them. But that would not be the only tendency: there would be a secondary tendency working the same way. For, after this fighting had gone on a year or two, it would become notorious among authors that if they published their books with small publishers they would be in danger of rival editions, in case of success, being issued by large publishers; but that, contrariwise, if they published with large publishers they would be in no danger of rival editions. Hence they would desert the small publishers; and in a double way the small publishers would lose their business. We should progress toward a monopoly of a few large houses; and the power which such have already of dictating terms to authors would become still greater.

Q. And if I understand you rightly, the power would be not only to dictate terms to authors, but of price to the public?

A. Yes, they would be able to combine. When you got a small number of publishers, and they could agree to a system of terms, the public would be powerless against them, and authors would be powerless against them.

Q. Then, in your opinion, is there any way by which works could be cheapened by legislative enactment?

A. There is one way, and that a way in principle exactly the reverse of that which is contended for in this measure; namely, the extension of copyright. I do not mean the extension in time; I mean the extension in area. On this point I am happy to say there appears to be agreement between the two sides. From the evidence which I have read I gather that it is proposed along with this limitation of copyright in time to extend copyright in area. I do not altogether understand the theory which, while it ignores an author's equitable claim to the product of his brain-work in respect of duration, insists upon the equity of his claim to that product of his brain-work, as extending not only to

his own nation but to other nations. However, I am glad to have agreement so far; and I hold, along with those who support the proposed measure, that the enlargement of the markets by means of international copyright would be a very effectual means of cheapening books. It would be a more effectual means of cheapening books than at first appears, and especially a means of cheapening the best books. I may refer again to this "International Scientific Series." One of the means by which that series has been made cheap was, that the American publisher and the English publisher agreed to share between them the cost of production, in so far as that the American publisher had duplicate stereotype plates and paid half the cost of setting up the type. Now it is clear that if the outlay is diminished by having one cost of composition for two countries instead of a cost for each, the book can be issued at a lower rate in both countries than it could otherwise be. And that arrangement, voluntarily made, under a kind of spontaneous copyright, in the case of the "International Scientific Series," would be forced, as it were, upon publishers in the case of an established copyright. Consequently there would be habitually an economization of the cost of production, by dividing it between the two countries; and hence there would be a lowering of the price. And then there is the further fact that this would tell especially upon the more serious books. On books of a particular kind the chief cost is for paper and print—large editions being printed. Therefore it does not so much matter in America having to set up the type afresh. But in the case of a grave book of which the circulation is small, the cost of composition is the main element in the cost; and the economization of that cost, by dividing it between England and America, would serve very considerably to lower the price.

Q. (*Dr. Smith*). Then, if I understand you aright, you do not approve of the principle adopted in the Canada Act, in the act passed by the Canadian Legislature of 1875, confirmed by the Imperial Act, by which it is necessary in order to obtain copyright in Canada that the works should be set up afresh?

A. I think that is obviously nothing else than a means of staving off the opposition of printers, and a very mischievous arrangement.

Q. Would it not be the fact that if a work could be set up once for all in one country, and circulate in the two countries, the price of the book would be diminished?

A. Unquestionably.

Q. (*Sir H. Holland*). You are aware of the difficulties that have been raised by the United States publishers: that constant attempts have been made ever since 1854 and before to make a copyright convention, and that there is no very great probability of these attempts proving successful. Have you any particular suggestion to bring before the commissioners which would in your opinion tend toward making the Americans favorable to a convention?

A. I am sorry to say I do not see my way toward any such suggestion. I was merely replying to the general question whether legislation could do anything to cheapen books, and saying that the only thing I thought it could do would be to get, in some way, an extension of area for copyright.

THE FORMATION OF MOUNTAINS.

PROFESSOR ALPHONSE FAVRE, of Geneva, has been making an interesting series of experiments to illustrate the formation of the great inequalities of the earth's surface by means of lateral thrust or crushing. These he describes and illustrates in a recent number of "La Nature," to which we are indebted for the illustrations which accompany this article. Professor Favre refers to the early experiments of Sir James Hall with various kinds of cloth, which he made to assume a variety of shapes by means of weights. He speaks of the various theories of the elevation of mountains, and especially of that of H. B. de Saussure, whose term *refoulement* seems to have meant much the same as that used by M. Favre, *écrasement lateral.*

"The three systems," M. Favre says, "which account for the origin of mountains by forces which push the great mineral masses from below upward, from above downward, or laterally, do not differ so much from each other as at first sight appears. Those geologists who have admitted the system of elevations as the principal cause of modification of the surface of the globe would probably enough admit the formation of depressions as a secondary modification; and so those who have accounted for these modifications mainly by depression, would probably enough also admit elevation as a secondary factor. Again, in the system of lateral crushing, there is a general depression of the surface of the earth, since there is a diminution in the length of the radius of our globe, and yet there result elevations of the ground in the midst of this general depression.

"The cause of lateral crushing," M. Favre goes on to say, "is owing to the cooling of the earth. It is, in fact, very probable that our globe is at the stage when, according to Élie de Beaumont, 'the mean annual cooling of the mass exceeds that of the surface, and exceeds it more and more.' It must follow that the external strata of the globe, tending always to rest on the internal parts, are wrinkled, folded, dislocated, depressed at certain points, and elevated at others.

"The experiments," M. Favre continues, "which I have made at the works of the Geneva Society for the manufacture of physical instruments, resemble much those of Sir James Hall; they differ notably, however, in two points: 1. The celebrated Scotchman caused the matter

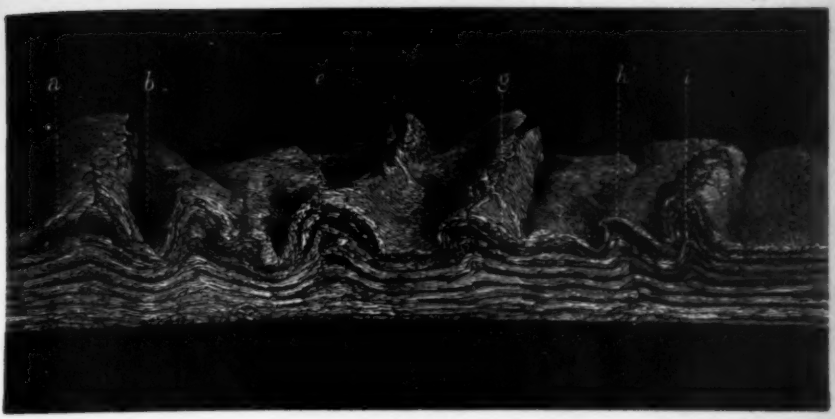


FIG. 1.

which he wished to compress to rest on a body which itself could not be compressed, while I placed the layer of clay employed in these experiments on a sheet of caoutchouc, tightly stretched, to which I made it adhere as much as possible ; then I allowed the caoutchouc to resume its original dimensions. By its contraction the caoutchouc would act equally on all points of the lower part of the clay, and more or less on all the mass in the direction of the lateral thrust. 2. Hall compressed, by a weight, the upper surface of the body which he wished to wrinkle, which prevented any deformation, while by leaving that surface free, I have seen, during the experiment, forms appear similar to those of hills and mountains which may be observed in various countries. . . .

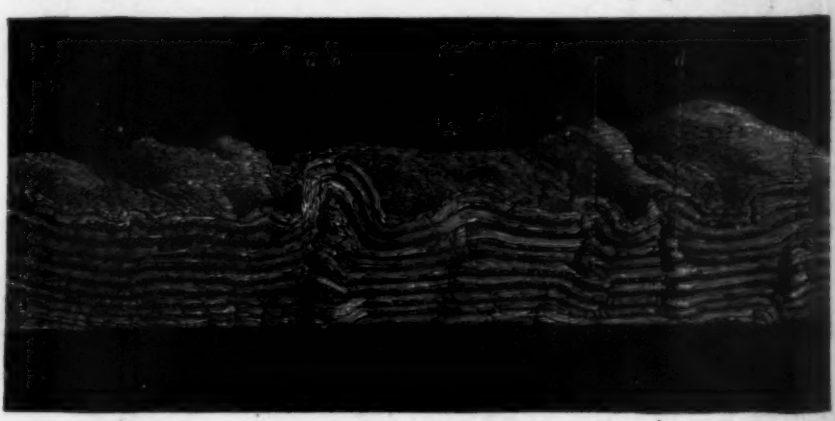


FIG. 2.

"The arrangement of the apparatus is very simple. A sheet of India rubber 16 mm. in thickness, 12 cm. broad, and 40 cm. long, was stretched, in most of the experiments, to a length of 60 cm. This was

covered with a layer of potter's clay in a pasty condition, the thickness of which varied, according to the experiments, from 25 to 60 mm. It will be seen from the dimensions indicated that pressure would diminish the length of the band of clay by one third. This pressure has been exerted on certain mountains of Savoy. For example, the section which I have given [1] of the mountains situated between the Pointe-Percée and the neighborhood of Bonneville enables it to be seen that those folded and contorted strata which are shown between Dessy and the Col du Grand Bernard cover a length which is two thirds of that which they had before compression. These mountains, then, have been subjected, like the potter's clay, to a compression indicated by the ratio of 60 to 40. Contortions are not, perhaps, observed over all the surface of the globe; it has not been equally folded in all its extent, but they are found in a great number of countries, and even beneath strata almost horizontal. Sometimes the folds approach the vertical, and are close against each other; this structure indicates that pressure has been exercised in a stronger manner than I have indicated.

"These powerful lateral thrusts of the external and solid parts of the globe appear to result from a diminution which the radius of the interior pasty or fluid nucleus has undergone during millions of ages. It may have been sufficiently great to cause the solid crust (which must always have been supported on the interior nucleus, whose volume continually diminishes) to assume the forms which we know, with a slowness equal to that of the contraction of the radius.

FIG. 3.

"To return to my experiments. At the extremities of the band of clay are pieces of wood or supports, which accompany it in its movement of contraction. The clay is thus compressed at once by its adhesion to the caoutchouc and by lateral pressure of the supports. By

[1] "Bulletin Société Géologique de France," 1875, t. iii., pl. xxii. A. Favre, "Recherches Géologiques," Atlas, pl. ix.

the influence of the caoutchouc alone, without the presence of the supports, there are formed only slight wrinkles on the surface of a sheet of clay 3 or 4 cm. in thickness ; and if the supports alone compressed the clay placed on a material which is not compressed (a very smooth oiled plate), the clay scarcely wrinkles near the center of its surface; it increases a little in thickness and forms swellings (*bourrelets*) against the supports. The strata which appear to divide the masses of clay, and which are represented in the figures, are not really strata, but simply horizontal lines at the surface of the clay."

Such pressure as has been applied in these experiments produces contortions of strata which elevate the surface of the matter compressed, as well in the plane parts or plains, as in those which take the forms of valleys, hills, or mountains. These latter have the appearance of vaults or folds, sometimes perpendicular, sometimes warped (*déjetés*) ; the ridges are complete, or broken at the summit by a longitudinal fracture, narrow below and wide above ; next, another fracture, narrow above and wide below, is produced at the base of the mountain or vault. The sides of valleys are sometimes almost vertical, sometimes present gentle slopes. The strata are less strongly contorted in the lower parts than in the neighborhood of the upper surface. They are disjoined in certain parts by fissures or caverns; they are traversed by clefts or faults inclined or vertical. All these defor-

FIG. 4.

mations are the more varied in that they are not similar on the opposite sides of the same band of clay.

Most of these phenomena are seen in Fig. 1, which represents the result of an experiment made on a band of clay whose thickness before compression was about 25 mm., while after that it attained 62 mm. at the culminating point. At *a* is seen a vault a little broken at the summit, covering a cavern similar to that figured in the memoir of Sir J. Hall

("Transactions R. S. E.," vol. vii., 1813), and to that of the Petit Bernard in Savoy (Favre, "Recherches," pl. x.); at *b* is a valley open at one of its ends and almost closed at the other; at *e* is a vault almost straight, the prolongation of which is very level; at *g*, *h*, and *l* are vaults twisted and a little broken, while at *i* is a broken fold, the curves of which are almost vertical. All these accidents of the ground recall those which have been so often observed in the Jura, the Alps, and the Appalachians.

Fig. 2 represents a band of clay whose thickness was about 40 mm. before compression and 65 after. We remark contortions similar to those of the preceding figure, among others a vault *a*, very exactly formed. At distances are seen vertical slices, on which the pressure appears to have acted in a particularly energetic fashion, and which may be called "zones de refoulement"; the strata are there broken in an exceptional manner, often separated from each other. One of these vaults is replaced by a single vault on the opposite side of the band of clay.

Before compression, in the band of clay in Fig. 3, were seen the two divisions which are seen there now—that in the right was 33 cm. long and 25 mm. thick at *a*, and 35 at *b*; the left division was 25 cm. long and 65 mm. thick. A gentle slope united the part *c* to the part *b*. After compression, the mean height of *a b* was 45 and that of *c* 75 mm. All the layers were spread horizontally.

"In this experiment I have sought to imitate the effect of crushing at the limit of a mountain and a plain. The height of the mountain *c* has been notably increased, the five or six upper layers have advanced on the side of the plain; they encroach on it. The plain has, however, offered a resistance sufficiently great to cause the strata of the mountain to be strongly inflected at the bottom. From this struggle between the plain and the mountain there resulted a cushion, *d*, which is the first hill at the foot of the height. It also resulted that the strata of the plain assumed an appearance of depression at contact with the mountain in consequence of the vault which is formed at *b*; they plunge underneath the mountain. This resembles what is often seen in the Alps at the junction of the first calcareous chain and the hills of 'mollasse'; in fact, the strata of the latter rock seem to plunge under those of the neighboring heights. In consequence of the pressure, there are formed several ranges of hills in the plain between *b* and *a*.

"In Fig. 4 the band of clay had, before compression, a thickness of 45 mm.; after that the culminating point was more than 10 cm. I have here sought to represent what must happen when terrestrial pressure is exerted on horizontal strata still moist, deposited at the bottom of a sea where are two mountains already solidified. For this purpose I placed in the caoutchouc and under the clay two bare cylinders of wood, *a* and *b*, of about 35 mm. radius, at 20 cm. from the ends of the

band of clay, and at the same distance from each other. Before compression the surface of the clay and the strata were completely horizontal. Pressure gave rise at the top of the half-cylinder, *a*, to a valley, *c*, formed by a twisting of the beds to the right, and by a little mountain, *d*, to the left. But I do not believe that it has ever been thought to assign to a valley an origin of this nature.

"On the other semi-cylinder, *b*, is produced an enormous elevation which has carried the ground to *e*, with such a rupture that the left lip, *f*, *g*, has suffered a complete reversal by turning, as on a hinge, around the horizontal line which passes by the point *h*. It follows that the four upper strata of clay designated by the figures 1, 2, 3, 4, being in a normal position before compression, are, after that, so arranged as to show the succession represented by the following arrangement of figures: 1, 2, 3, 4, 4, 3, 2, 1, 1, 2, 3, 4, making the section of this formation by a line drawn from *x* to *z*. If the left lip should disappear we should then have between the points *x* and *z* the section 1, 2, 3, 4, 5, 1, 2, 3, 4, 5. Sections analogous to these, presenting inversions in the order of strata, are known to geologists.

"The forms assumed by the clay depend on several circumstances which it is difficult to describe, such as the strength and the rate of compression, the thickness and the greater or less plasticity of the clay, etc. Why have accidents of the upper surface of the clay, which are intimately connected with those of the interior of the mass, so small an extension that they are not even similar in the two sides of a band of clay? This small continuity is owing to causes which we can neither foresee nor appreciate. Is it not the same in nature? Why is the chain of the Alps not a true chain, but a succession of masses often oblique with respect to each other? Why, in the Jura, do we see chains which have for their prolongation plains and valleys? It is always the case that the forms and structures obtained in these experiments have an incredible resemblance to those which are found on the surface of the globe. But it must be admitted that many of the latter have not been reproduced by these artificial crushings.

"It appears probable that, by pressures more powerful and more variedly employed, we might obtain again very different structures. But I have not thought it necessary to multiply these experiments, thinking that the varied forms which have resulted show sufficiently the effects of crushing."—*Nature.*

PLANETARY RINGS AND NEW STARS.

By Professor DANIEL VAUGHAN.

THE study of celestial phenomena which represent changes of the greatest magnitude is an important source of intelligence respecting the course of creation and the diversified condition of the universe. In all well-directed efforts to explore the more profound secrets of space and of time, it is necessary to be guided by certain conspicuous marks which even unseen planets may leave behind them, on assuming new forms or in closing an inconceivably long term of existence. Within the solar domain there is, perhaps, no object which claims more interest or value for original inquiries in astronomy than Saturn's rings—whether they be regarded as presenting a picture of the first or the last stages of planetary life. With a wide deviation from the ordinary figure of worlds, they exhibit changes which are interesting on account of the vast scale on which they occur, and the light which they throw on the past and the future history of the solar system. In the absence of those restraints which secure much repose on the surface of our globe, the Saturnian girdle is abandoned to rage of the most violent commotions and becomes occasionally the seat of disturbances which, though transpiring at the distance of about 900,000,000 miles, are yet revealed by the telescope. The temporary divisions which have been so often observed in the rings are evidences of the terrific scenes of turmoil in this remote theatre of chaotic activity; as the opening and closing of visible chasms must be attended with physical convulsions immeasurably greater than any ever witnessed on our terrestrial abode.

From well-established principles of physical astronomy, it is easy to prove the impossibility of tranquil movements in a region so close to the great planet. Whether tenanted by innumerable solid masses or even by a vast expanse of fluid, the zones in which the Saturnian sway is so powerful must present a long-continued struggle for opposite ends. The matter spread over the wide annular fields is ever urged by its own attraction to collect together and form satellites, which are soon destroyed by the attractive disturbance of the primary, and have their parts scattered once more over a wide space. From the gravity due to their preponderating masses, the mountains or inequalities observed on the ring can not be prevented from growing at the expense of the matter along the zone in which they circulate; but with the increasing size the vast structures become at last incapable of sustaining the crushing strain in certain directions; so that a dilapidation and a dispersion of their materials become inevitable. It is in consequence of the ephemeral character of these mountains or embryonic satellites, that observations on them have sometimes led as-

tronomers to absurd and discordant conclusions in regard to the general movements and the stability of the rings.

With the resources which spectrum analysis has conferred on science, more advantages are to be expected from inquiries in regard to the exact nature of the matter of distant space; and the basis for opinions or conclusions in regard to the composition of celestial objects is more then usually favorable in the case of Saturn's bright rings. That they are not entirely liquid or gaseous is evident from their serrated edges shown in the observations of Trouvelot, and from the peculiar character of the inequalities observed during their disappearance by Lassell and other astronomers. And yet there are few solid substances which could endure the long course of turmoil and ruin without being reduced to powder, and thus rendered incapable of raising mountainous structures high enough to be visible from our earth. But if the great annular appendage were largely or wholly composed of water with a temperature near 32° Fahr., the readiness of the fluid to assume a frozen condition would be a remedy to the ceaseless work of destruction, and would give solidity enough to enable incipient satellites to rise to the height of more than one hundred miles before tumbling to pieces.

The range of temperature necessary for the continuance of such operations must be maintained chiefly by the thermal effects with which they are attended. Any large stock of primitive heat which (according to the more generally received opinions) the rings might have possessed at their origin, must have been long since wasted by radiation from their extensive surfaces. In that remote and frigid zone of our planetary domain, the rays of our sun are too feeble to mitigate the rigors of extreme cold; and the outer ring at least can obtain but little calorific relief from the great planet which it environs. But, from the incessant changes and convulsions in the restless fields of matter, heat is abundantly produced by the violent mechanical action which, in a ring of aqueous composition, would proceed in a manner calculated to give a uniformity of temperature. If such a half-frozen ocean were abnormally heated throughout much of its vast expanse, so that a large portion of its ice were liquefied, the water, on obtaining a preponderance, would perform its movements and fluctuations with less violence and loss of living force. The heat produced mechanically would be then less than the amount lost by radiation, and a return of cold would again give ice the ascendancy. Yet, as the temperature declined and the freezing extended, mechanical violence would again become more energetic; and heat would be more copiously developed by the collisions of icy blocks, and by the rise and fall of gigantic mountains.

Reasoning from the most reliable principles of physics, and guided by the light of recent discoveries, many eminent scientists have come to the very just conclusion that the movement in Saturn's rings must be attended with a loss of energy and a reduction in the size of the orbits

described by the innumerable disconnected masses of which the curious appendage is now generally believed to be composed. The opening and closing of its chasms, and other observed signs of its restless character, are calculated to give the impression that the extent to which it is altered during many years must be considerable. But, considering the conditions necessary for the phenomena it exhibits, it would seem that the rapidity with which its changes proceed might be approximately determined from the amount of heat which it radiates into space, and which must have been, for the most part, produced at the expense of its motion. Now, though the rings may be principally composed of water, and have a superficial temperature near the freezing-point, yet, their surfaces being over a hundred times as extensive as that of the earth, they may be reasonably supposed to lose by radiation about thirty times as much heat as our planet receives from the sun and allows to escape into space. Taking Bessel's estimate for the mass of the double girdle, it will be found that such an amount of heat might be generated hy the conflicting movements of its parts without reducing their orbits more than one per cent. in ten thousand years. There is, indeed, reason to believe that, in this case, Bessel's results are unreliable, in consequence of the uncertain and defective character of the data with which they were obtained. If we assign to each ring the probable amount of matter in the neighboring moons of the gigantic planet, it would seem that their permanent change of size may be so considerable that it might be detected by the observations of a few centuries.

It is likely that, in the inner ring, especially at the zone nearest to the primary, the temperature is much higher than that which I have supposed, and that alterations in its condition might proceed at a rate sufficiently rapid to be discoverable by the telescope. More than twenty years ago Otto Struve, having carefully compared observations since the time of Huygens, announced as the result of his labors that the inner ring is changing its dimensions so rapidly that before two hundred years it will be united to the planet. Other astronomers have expressed a belief of the recent origin and of the mutable nature of the obscure or vapor ring which lies closer to Saturn. The conclusions of Struve, however, have been disputed; and indeed it is probable that they give an exaggerated picture of the transitory state of things in the Saturnian dominions; nor can the conflict of opinions on this point be settled by observation alone. But, though taking place too slowly to be at once detected in this way, the changes in question are still inevitable ; and they give safe ground for tracing the history of past events in this part of the celestial regions. It is evident that the matter composing Saturn's wonderful appendage must have once moved in a wider zone, where it could exist only in the form of two secondary planets. I have shown in a previous article that a dismemberment and a conversion into a ring must be the general fate of every planetary body which, by a slow contraction of its orbit, revolves at last too close

to its primary. From these and other considerations it may be legitimately concluded that the rings of Saturn are the remains of two former satellites; and their origin and their present condition must be regarded as the ultimate consequence of a rare medium disseminated through space.

Though the dismemberment which I have shown to be inevitable in small orbits differs, in some features, from that supposed to take place in the nebular hypothesis, yet in tracing the effects of both it is necessary to be guided by certain mathematically demonstrated principles in regard to stability. Two homogeneous fluid planets varying widely in size, but having the same density and occupying the same time in their diurnal movement, would be similar spheroids, or have the same relative deviation from a true sphere. Stability would cease to be possible in both if they were as dense as the earth and turned once in two hours and twenty-five minutes. If they were as rare as hydrogen gas at the level of our seas, they could not endure a rotation which took place in a less time than twenty-five days. In the investigations which I have given in the "Philosophical Magazine" in regard to secondary planets close to their primaries, the results have a like generality. Twelve hours would be very nearly the shortest time of revolution for an homogeneous fluid satellite as dense as water, whether its diameter were a hundred or a thousand miles, or whether it revolved around the earth or around Jupiter. Such a body, however, would require to be about thirteen times as dense as its primary in order to circulate in safety a little beyond the surface of the latter orb. A small satellite composed of fluid quicksilver would be capable of maintaining a planetary form if revolving just outside the atmosphere of Saturn or of Neptune; but it would be doomed to dismemberment if moving in a similar proximity to the surface of the earth or even of Jupiter.

The results are not very different even in cases of the greatest possible deviation from the homogeneous character I have ascribed to the bodies. If both planet and satellite were composed of rare gas enveloping a central nucleus, the smaller body would require to have an average density nearly eight times that of the greater, in order to preserve its integrity in such a dangerous proximity. A modern advocate of the nebular hypothesis supposes that each planet, when formed from the rarefied matter of a previous solar ring, was fifteen times less dense than the sun would be if it were an homogeneous sphere inclosed by the planetary orbit. In his own words, "After all their contraction during their condition as rings, and during their aggregation into globes, we may assume at a moderate estimate that when their rotation began they were fifteen times less dense than the average density of the sun expanded to their orbits." To change from a ring to a planet, however, the nebulous matter should have about one hundred times the density which the writer ascribes to it. Before it could become dense enough for the transformation, the nebulous expanse would, like

Saturn's rings, be a prey to great commotions, sustaining a constant loss of energy and contracting the circles which its parts described around the sun. In endeavoring to account for the direct motion in secondary systems, Laplace contends that, in consequence of friction, the supposed primitive solar rings would have a greater velocity in their outer than in their inner zones. Now, if friction were so potent as to counteract to such an extent the normal effects of gravitation, it must be an eternal bar against the origin of worlds by nebulous dismemberment; and if a ring of attenuated matter were placed under the circumstances suggested by the eminent astronomer, it would be ultimately doomed, not to form a planet, but to coalesce with the immense spheroid of fiery vapor which it is supposed to have environed.

For further progress in the task of tracing the course of celestial events and thus obtaining materials for an astronomical history of worlds, it is necessary to consider, not only the theory of motion and stability in comparatively small orbits, but also the effects which a resisting medium of space may produce during long periods of time. Though the evidence which cometary motion gives of this rare fluid is far from being satisfactory, more reliable information on the subject may be gleaned from other sources, as I pointed out in an article in the "Philosophical Magazine" for June, 1861. In the September number of "The Popular Science Monthly" I alluded to the form of Mars as bearing marks of a former rapid rotation which appears to have been considerably reduced by the friction of a space-pervading fluid. But by far the most acceptable evidence on this question has been lately derived from the peculiarities of the nearest moon of Mars, as the small size of its orbit and its brief period of revolution have been ascribed to a resisting medium, even by advocates of the nebular hypothesis. The bearing of the new discoveries on certain astronomical doctrines has been already pointed out, in the July number of this journal, by my long-esteemed friend whose useful life has been since lost to the cause of science. The effects of the space-pervading fluid will appear more decided if we regard the diminutive satellites as former asteroids which became so far a prey to the Martian attraction as to be reduced to their present subordinate condition. Such views respecting their origin were first suggested by Prof. Kirkwood, and were subsequently advocated by Prof. Alexander in a paper read before the National Academy of Science.

The possibility that some straggling fragments from the ruins of one world might become the satellites of another, depends on a principle which has been long applied in tracing the origin of the cometary members of the solar family. It has been maintained by Laplace that comets were at first strangers in our system, and that many of them, coming from remote interstellar regions and entering the sphere of the sun's attraction, obtained a permanent domicile in his extensive domain; their orbits being changed from hyperboles to ellipses chiefly by some planetary disturbances. There would, however, be less probability that

a small asteroid would become a satellite to Mars on passing through the region over which his attraction is effective; as the change would depend mainly on the intervention of solar influence, and would require rare conditions which the movements of many hundred asteroids could furnish only during long periods of time. But it may be considered certain that a moon obtained in this way must have had its primitive path so very extensive, that its revolution occupied a term of several months. Accordingly, if the asteroidal origin of the Martian moons be adopted, their present condition must be indicative of greater deviations from their primitive arrangement; and it is evident that their early large orbits could be reduced to the present small size only by a resisting medium. This theory is, perhaps, not wholly proof against all objections, but another which accounts better for the movements of both small secondaries in the plane of the equator of the planet, and which I intend to set forth in a future article, involves also the necessity of supposing the imperfect vacuity of celestial space.

As, in traversing a rare medium, such small masses must lose more velocity in one year than our globe would in many centuries under the same circumstances, they afford the most available means for indicating the slow alterations in the state of the universe. In the absence of the diversified cases which are, no doubt, concealed from our knowledge in distant solar systems, the small moons will serve as the means of illustration of the ultimate effects of the slight but constant resistance to celestial motion. It is evident that not many million years can elapse before Phobos will have its orbit so far reduced that it will sweep through the atmosphere of Mars, and then its career as a small secondary world will close with a meteoric exhibition. A term of existence several hundred times longer must be ascribed to our moon, or to the first satellite of Jupiter; but their end would be signalized by a far greater display of meteoric effulgence. A secondary planet two or three thousand miles in diameter, even if solid, would become unstable before coming in contact with its primary, and would undergo a sudden dilapidation; so that a numberless host of its fragments, scattering into smaller orbits and plunging as meteors into the atmosphere of the great central orb, would send forth a flood of brilliancy sufficient to rival solar light, and to proclaim the great work of destruction to the most distant parts of the universe.

The paroxysmal manifestation of light which a planet could scarcely fail to call forth in thus passing through its final stages of existence, corresponds in every feature to the mysterious effulgence of temporary stars. The accordance of the theory with facts appears more satisfactory, in proportion as the problems involved in the inquiry are more accurately solved with the aids of mathematics, and as new means of observation reveal the true nature of these rare and transitory apparitions of stellar light in the skies. The theatre of one of the great meteoric exhibitions in question may be the aërial envelope, not only

of a large planet or a vast dark central body presiding over unseen planetary members, but even the corona and the photosphere of a sun. Whenever any of the large members of a solar or of a secondary system become unstable in too small an orbit, a vast portion of its dilapidated mass would be quickly sweeping as innumerable meteors through the atmosphere of the immense primary sphere. This accounts for the incipient brilliancy of the temporary stars, a fact hitherto unexplained, though it is generally admitted, and though it has been recognized by a high authority as a valuable guide in the study of these mysterious phenomena. "The circumstance," says Humboldt, "that nearly all the new stars burst forth at once with extreme brilliancy as stars of the first magnitude, and even with still stronger scintillations, and that they do not appear, at least to the naked eye, to increase gradually in brightness, is in my opinion a singular peculiarity, and one well deserving of consideration." Recent discoveries, though calling for some modification in this statement, detract little from its value; for the three new stars of the present century, though all below the first magnitude, yet showed their greatest effulgence at an early period of their visibility, and afterward exhibited a constant decline. According to the present theory, a rapid weakening of brilliancy in these objects would be an inevitable result: as a large portion of the meteors must have been successively precipitated to the surface of the great central sphere; while the balance assumed a closer array, changing their orbits into circles and forming a solar or a planetary ring.

The most favorable circumstances for such sudden outbursts of light are presented in cases where, in mass and size, the subordinate world is little more than one per cent. of the solar or the primary orb with which it is doomed to incorporate. If, for instance, our moon were caused to revolve so near us that it would be rendered unstable by terrestrial attraction, its dismemberment, though occurring on a large scale, would be confined to the region nearest to the earth. A vast portion of the lunar matter torn from this locality would be hurled to our globe or would fly as innumerable meteors through our atmosphere. But the remainder of our satellite would retire to a greater distance from the earth; and millions of centuries would elapse before it became again close enough to our world to suffer another great dilapidation and to give occasion for another gigantic display of meteoric light. It would thus appear that many great luminous exhibitions would attend the awful paroxysms with which a large planet passes away from the stage of existence in a solar or in a secondary system. Though a small satellite, if fluid, may meet its final doom in an obscure manner, yet, if solid, it would be likely to maintain a planetary form until it came very close to the primary; so that on its dilapidation a large portion of the resulting fragments would sweep through the atmosphere of the latter and call forth a sudden effulgence which in very remote worlds would appear as the transitory glare of a temporary star in the firmament.

In carefully tracing the conspicuous scenes which must mark the end of a planetary career, and thus obtaining a more correct interpretation of the rare and mysterious characters occasionally inscribed in our skies, very interesting information may be obtained of the diversified contents of space, and the long term of existence assigned to each of the numerous worlds of creation. If the dominions of other suns be equally rich as our solar region in mundane objects, it may not be extravagant to suppose that, in our universe, the large primary and secondary planets enlivened by the genial influence of more than twenty millions of stellar bodies might equal in number half the population of our globe. Now, the average mortality in the human family is about one death every second, while astronomical records show that only twenty-three temporary stars appeared within the past two thousand years. Taking their appearance as records of planetary fate, it would follow that a century is as small a part of the career of a planet as two seconds is of human life; and that the few thousand years in which the history of our race is comprised is scarcely two minutes in the immeasurable age of our world.

Yet these considerations will perhaps give an inadequate idea of the long endurance of the great works in creation's wide domain. According to the opinions of Laplace, besides the systems over which visible stars preside, there are others equally numerous in which the central bodies, though of sunlike magnitude, are not self-luminous. Mädler and Bessel embraced similar views. Those who believe, with Helmholtz, that a sun's heat and light are produced by the contraction of its mass, and that solar activity has a limited duration, might be naturally led to consider dark systems a hundred or even a thousand times as numerous as those which are illuminated. Yet I think it more reasonable to take the moderate estimate of Laplace for the comparative numbers of the dark and the bright occupants of space. But it is, moreover, necessary to consider that great planets and satellites meet their ultimate doom by a number of dismemberments and great meteoric scenes, each separated by intervals of many millions of centuries. Taking all these circumstances into account, the age of a world, as inferred from the observed indications of catastrophes in the heavens, may reach as high as 500,000,000,000 years.

If the feelings of some readers will revolt from the idea of having a primary or even a secondary mundane orb occasionally sacrificed in some part of the wide celestial domain, they must be powerfully shocked by the views of Dr. Croll, who gives destruction a far more oppressive sway over the great works of creation, when he regards the collisions of suns as the normal means of perpetuating the economy of nature. If the great centers of unfailing light were thus hurled into ruin, their attendant worlds, if saved from a worse fate, would be sent adrift in hyperbolic orbits and doomed to a long pilgrimage in the cold interstellar regions. Without denying the possibility of such rare and

terrific convulsions, I must pronounce it as certain that they can not have any part in giving birth to the new stars which astonished Tycho Brahe and Kepler, or to those which have blazed forth in the heavens in our own times. Though the greater cosmical bodies might, as Dr. Croll supposes, be heated by collisions so intensely as to be capable of diffusing heat and light for many millions of years, they could not undergo the rapid decline of brilliancy which temporary stars exhibit. Lockyer takes similar grounds in speaking of Nova-Cygni. "We are driven," says he, "from the idea that these phenomena are produced by the incandescence of large masses of matter, for, if so produced, the running down of brilliancy would be exceedingly slow." A planetary wreck, incorporating with the sun in the manner I have described, would sweep through his external matter at the rate of about two hundred and eighty miles a second. The heat produced mechanically at the expense of this high velocity would not be so great in quantity as that which might be expected from Dr. Croll's solar encounters; but, being confined to a very limited zone, it would attain much greater intensity, be more effective for dissociation, and prove a more efficient means for giving nebulæ their existence and the peculiar character which they exhibit.

Since spectrum analysis has been brought to bear on the new stars, the doctrine of their meteoric origin has obtained more currency in astronomical circles. Though the incorporation of a remote world with a greater sphere around which it previously revolved has been suggested as the cause of such meteoric action, the idea has been somewhat unproductive, in consequence of the loose manner in which inquiries on the subject have been conducted, and the little care which has been taken for obtaining correct solutions for the problems of motion and stability involved in the questions at issue. The consequences of instability and dismemberment in small orbits have been generally overlooked. Recent developments, however, show some steps for correcting the early errors and oversights in this new field of investigation. In his recent work on "The Struggle for Existence in the Heavens," Du Prel (alluding to the ultimate doom of the earth near the center of our system) states that our world will end its career, not as one gigantic meteor, but as numberless meteoric fragments; and then the great shower of stones in the solar atmosphere will show the inhabitants of some very remote orb such a spectacle as was to be seen by terrestrial astronomers in the constellation of Corona on the 12th of May, 1866. A similar conclusion has been expressed frequently in my writings during the past twenty-five years, especially in my papers published in the reports of the British Association for 1857 and 1861, and in my communications in the "Philosophical Magazine" for 1858, 1861, and 1872.

The meteoric phenomena of distant space may be profitably studied in connection with those to be seen on a diminutive scale in our at-

mosphere. The visits of shooting-stars to the earth have been lately brought within the province of astronomy, and numbers of these bright objects, which take part in extraordinary showers, have been found to come from the tracks of certain comets. It would, accordingly, seem that cometary bodies have portions of their matter separated from them, and occasionally sent as meteors into the atmospheres of the planets. The dismemberment which, in such cases, is occasioned by the heat, and more rarely by the attraction of the sun, is analogous to that which planets would suffer in very narrow orbits; but it occurs on a scale infinitely smaller, and can never be productive of any very conspicuous results. The greatest exhibition of shooting-stars in our atmosphere could never be observed from any of the neighboring planets; and, if armies of meteors were sent from many systems to invade a single one, and had their orbits and positions best arranged for a simultaneous charge on the atmosphere of one of its larger orbs, the light which they could produce would fail to exhibit the remarkable features observed in incipient brightness and the gradual decline of temporary stars.

THE OLD PHRENOLOGY AND THE NEW.

By Dr. ANDREW WILSON.

THERE has ever lain a strange fascination, for culture and ignorance alike, in the attempt to diagnose the intellect and character of man from the outward manifestations of his face and skull. The problem of character and its interpretation is as old as Plato, and may probably be shown to be more ancient still. Egyptian soothsayers and Babylonian astrologers were hardly likely to have omitted the indexing of character as a profitable and at the same time legitimate exercise of their art. The forecasting of future events and the casting of nativities were studies likely enough to bear a friendly relationship to the determination of character from face, from fingers, or from skull and brain itself. But the histories of palmistry and soothsaying, with that of physiognomy, are they not all writ in the encyclopædias? We shall not occupy space with an historical *résumé* of the efforts of philosophy in swaddling-clothes attempting to wrestle with the great problem of mind and matter; nor shall we at present venture to oppose a scientific denial to Shakespeare's dictum that

> . . . . there's no art
> To find the mind's construction in the face.

Darwin's "Expression of the Emotions," the development of facial contortions, and the interesting study of the genesis of smiles and tears, and of the thousand and one signs which make up the visible and emo-

tional life of humanity, may form a subject for treatment hereafter. Our present study concerns the deeper but not less interesting problem of the indexing of mind, and of the relations of brain-conformation and brain-structure to character and disposition. If there exists no art "to find the mind's construction in the face," Lavater notwithstanding, may we discover "the mind's construction in the skull"? If the old phrenology, or the science of brain-pans, be regarded as practically obsolete among physiologists and scientific men at large, what hopes of successfully estimating the "ecinage of the brain" may the new phrenology be said to hold out? To this interesting question, then, let us ask the reader's attention for a brief period. We may premise that, if the march in ways phrenological be somewhat bellicose, our journey shall not be wanting in those mental elements which make for instruction in a field largely peopled with human hopes and fears.

The professions of phrenology are not by any means so correctly appreciated as might be thought, considering how well known is the name of the science, and how popular were its tenets within, comparatively speaking, a few years back. Although the name "phrenology" is but an echo in the scientific class-rooms, its professors still flourish, mostly in obscure localities in large towns, and often present themselves as modern representatives of the Peripatetici, in that they wander from town to town as traveling philosophers who usually unite a little electro-biology to their phrenological talents, and throw in an occasional mesmeric *séance* by way of offset to the more serious business of the interpretation of character. There are, it is true, phrenological societies and museums in several of our cities. The latter are chiefly remarkable for the varied collection of murderers' effigies and for the extensive assortment of casts of cranial abnormalities; the exact relationship of these contorted images to phrenological science being rarely if ever made clear to the visitor on the search for knowledge. Now and then in opticians' windows one sees a wondrous china head whose cubic capacity is mapped off into square inches, half inches and quarters, of veneration, ideality, comparison, benevolence, and many other qualities of mind. The contemplation of such a work of art excites within the mind of the ingenuous observer an idea of the literal awfulness of a science which dispenses destructiveness by the inch, and which maps out the bounds of our amativeness by the rule of three; while the profundity of its professors may by such a mind be compared only to that of Butler's *savant* who

> . . . . could distinguish and divide
> A hair, 'twixt south and southwest side.

Nor would the admiration of the ingenuous one be lessened were he to enter the sanctum of the "professor" of phrenology, and submit his cranium to the ocular inspection and digital manipulation of the oracle. The very furnishings of the apartment are mystic, and impress

or overawe the inquiring mind. Pope's dictum concerning "the proper study of mankind" embellishes the walls; and the advice "know thyself," meant to be interpreted and taken in a phrenological sense, is given gratis through the medium of a conspicuous and usually illustrated poster. The tattooed head of a New-Zealander; a few skulls, occasionally supplemented by a collection of stuffed lizards and other reptilian curiosities, and invariably flanked by busts of the ancient philosophers, complete the æsthetic furnishings of the modern temple of the delineator of character. To the proprietor, in due time, enters a certain moiety of the British public in search of knowledge. And thence issue the patients, each provided for a consideration with a wondrous chart of their mental disposition, wherein the moral quicksands are presumed to be duly marked, and the obliquities of character stamped, with a view toward future correction and improvement.

How does the phrenological professor succeed very fairly in reading character? may be asked at the outset by readers who have had those parts of their disposition best known to themselves delineated with accuracy by the oracle. The reply is clear. Not through manipulating those mysterious "bumps," nor through any occult knowledge of the brains of his votaries, but simply from a shrewd talent for scanning the personal appearance and physiognomy of his clients, and by the dexterous suggestion of queries bearing on those traits of character which the features and manner reveal. Your successful phrenologist is in truth a shrewd physiognomist. His guide to character is in reality the face, not the brain-pan. The dress, manners, and deportment of his clients, and not the gray matter of the cerebrum, form the real basis of his observations. If any one may be found to doubt how accurately one's character may be mapped out from its outward manifestations, let him endeavor to study for a while the acts and deportment of those with whose "mind's construction" he may be even slightly acquainted, and he will speedily discover numerous clews to the mental disposition in common acts and traits which previously had passed utterly unnoticed. Such a result accrues speedily to the professed physiognomist and shrewd observer of men, who, passing his fellows in professional review before him, speedily discovers types of character to which, with allowance for special proclivities or traits, his various clients may be referred. That character may with tolerable success be determined even from handwriting is a well-known fact; and it is difficult to see the superiority of the pretensions and claims of phrenology as a guide to character over those of the professor of calligraphic philosophy. One of the most convincing illustrations that even a practical knowledge of brain-structure is not necessary for the successful delineation of such superficial traits of character as can alone be determined by the casual observer, may be found in the fact that very few "professors" of phrenology have ever studied the brain, while a large proportion may never have seen an actual brain. A notable example of a successful practice of phrenology

being carried on independently of any knowledge whatever of the brain, is known to the writer, in the case of a worthy police-sergeant, who attained tolerable accuracy in the art of reading "the mind's construction," but who had never even seen a brain, and who had the faintest possible idea of the appearance of that organ. Unless, therefore, one may logically maintain that total ignorance of the brain-pan is compatible with an accurate understanding of its contents and mysteries, the successful practice of phrenology must be shown to depend on other data and other circumstances than are supplied by anatomy and physiology—these sciences admittedly supplying the foundation of all that is or can be known regarding the brain, its conformation, structure, and functions. Empirical science—science falsely so called—will not hesitate to assert its ability to accurately solve the deepest problems of character and mind. But the more modest spirit of the true scientist will hesitate before crediting itself with any such ability, or even before giving assent to such general rules of character as are exemplified by the saying, "Big head and little wit"; or by that of the worthy Fuller, who, in his "Holy and Profane State," remarks that "often the cockloft is empty in those whom Nature hath built many stories high."

The fundamental doctrine of the old phrenology is well known to most of us. Its great doctrine is pictorially illustrated in the china heads of the opticians' windows, and may be summed up in the statement that different parts or portions of the brain are the organs of different faculties of mind. The brain thus viewed is a storehouse of faculties and qualities, each faculty possessing a dominion and sphere of its own among the cerebral substance, and having its confines as rigidly defined as are the boundaries of certain actual provinces in the East, the status of which has afforded matter for serious comment of late among the nations at large. Thus, if phrenology be credited with materializing mind in the grossest possible fashion, its votaries have themselves and their science to thank for the aspersion. If it be maintained that feelings of *destructiveness* reside above the ear, then must we localize the desire to kill or destroy in so much brain-substance as lies included in the "bump" in question. When vainglory besets us, we must hold, if we are phrenologists, that there is a molecular stirrage and activity of brain-particles beneath a certain bump of "self-esteem" situated above and in front of the ear; while feelings of veneration, of hope, or of wonder, are each to be regarded as causing a defined play of action in particular bumps and special quarters of the brain. Were the deductions of phrenology true, or were its claims to be regarded as a science founded on definite grounds, mind could no longer be regarded as a mystery, since it would be within the power of the phrenologist to assert that, when swayed by emotions of one kind or another, he could declare which part of the brain was being affected. This declaration logically follows upon that which maintains the localization of faculties

in different parts of the brain; but it is a conclusion at the same time from which Physiology simply retires in outspoken disdain, as presenting us with an empirical explanation of mysteries to which the furthest science has as yet failed to attain.

That we may duly understand, not merely the falsity of the old phrenology, but the bearings of the new aspects of brain-science as revealed by modern physiology, we must briefly glance at the general conformation of the brain. The organ of mind, contained within the skull, consists of the greater brain or *cerebrum* (Fig. 1, *A A*), and the lesser brain or *cerebellum* (*B*). The latter portion is situated at the

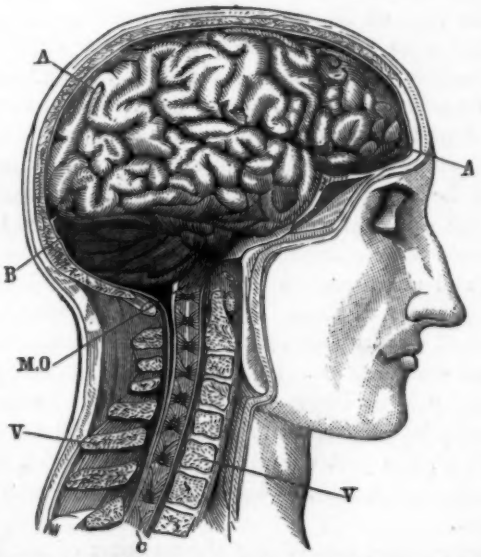


FIG. 1.—SIDE VIEW OF THE BRAIN AND SPINAL CORD.—(From Bourgery.) *A*, the cerebrum; *B*, the cerebellum; *M.O*, the medulla oblongata; *V*, the vertebræ, cut in halves; *C*, the spinal cord, and the origin of its nerves.

back of the head, and forms the hinder part of the brain; the spinal cord (*C*), which, as every one knows, runs through the spine (*V V*), being merely a continuation of the main axis of the nervous centers of which the brain is the chief. When the surface of the human brain is inspected, it is seen to present a very unequal appearance, due to the fact that its substance is thrown into a large number of folds or *convolutions* (see Fig. 1), as they are technically named by the anatomist. The brain, or cerebrum, is in reality a double organ, formed of two similar halves or *hemispheres*, which are separated by a deep central fissure, but which are also connected together below by a broad band of nervous matter known as the *corpus callosum*. It is this latter band which brings the halves of the brain into relation with one another, and which thus serves to produce identity and correlation of action between its various parts.

To the nature of the convolutions our especial attention must be directed. The brain-substance consists of gray and white nervous matter. The gray matter forms the outermost layer of the brain-substance, and incloses the white; the opposite arrangement being seen, curiously enough, in the spinal cord. Now, one evident purpose of the convolutions of the brain is to largely increase the amount of its gray matter relatively to the space in which the organ of mind is contained; while the perfect nutrition of the brain is also thus provided for through its convoluted structure permitting a fuller distribution of the minute blood-vessels which supply the brain with the vital fluid. It is a very noteworthy fact that the structure of the gray matter differs materially from that of the white. In the gray matter nerve-cells are found in addition to nervous fibers, the former originating nervous force, while the latter are simply capable of conveying this subtile force. Thus it may be said that it is in the gray matter that thought is chiefly evolved, and from this layer that purposive actions spring. The white matter, on the other hand, merely conveys nerve-force and nervous impressions, and is thus physiologically inferior in its nature to the gray substance. The observations of Gratiolet, Marshall, and Wagner seem to leave no room for doubt that the convolutions of the brain increase with culture, and are therefore more numerous and deeper in civilized than in savage races of men. It is curious, however, to observe that certain groups of quadrupeds are normally "smooth-brained," and possess few or no convolutions. Such are rats, mice, and the rodents or "gnawing" animals at large, and it can hardly be maintained that in those animals intelligence is normally low or instinct primitive—although, indeed, the just comparison of human with lower instincts must be founded on a broader basis than is presented by this single anatomical fact.

A final observation concerning the anatomy of the brain relates to its size and weight as connected with the intelligence. The phrenological doctrine of the disposition of the faculties must be held to include the idea that the larger the brain, the better specialized should be the mental qualities of the individual; the greater the amount of brain-substance forming the good and bad qualities and regions of the phrenologist, the more active should be the mental organization. Now, it is a patent fact that this rule tells strongly against the phrenologist's assumption. True, various great men have had large brains; but cases of great men possessing small brains are equally common, as also are instances where insanity and idiocy were associated with brains of large size. The normal average human male brain weighs from 49 to 50 ounces; man's brain being ten per cent. heavier than that of woman. Cuvier's brain weighed 64½ ounces; that of Dr. Abercrombie 63 ounces; that of Spurzheim, of phrenological fame, 55 ounces; Professor Goodsir's brain attained a weight of 57¼ ounces; Sir J. Y. Simpson's weighed 54 ounces; that of Agassiz 53·4 ounces; and that of Dr.

Chalmers 53 ounces. As instances of high brain-weights, without corresponding intellectual endowment, may be mentioned four brains weighed by Peacock, the weights of which varied from 67·5 to 61 ounces. Several insane persons have had brains of 64½ ounces, 62 ounces, 61 ounces, and 60 ounces, as related by Bucknill, Thurnam, and others. With respect to the brain-weights of the fair sex, anatomical authority asserts that in women with brains weighing 55·25 ounces and 50 ounces, no marked intellectual features were noted. Below 30 ounces, the human brain becomes idiotic in character, so that there appears to exist a minimum weight, below which rational mental action is unknown. The anatomist's conclusions regarding brain capacity and mental endowments are therefore plain. He maintains that the size and weight of the organ ,do not of themselves afford any reliable grounds for an estimate of the mental endowments, while his researches also prove that a large brain and high intellectual powers are not necessarily or invariably associated together.

The foregoing details will be found to assist us in our criticism of the pretensions of the old phrenology as a basis for estimating "the mind's construction" and the mental habits of man. Primarily, let us inquire if development—that great criterion of the nature of living structure—lends any countenance to the idea that the brain is a collection of organs such as the phrenologist asserts it to be. The brain of man, like that of all other backboned animals, appears to begin its history in a certain delicate streak or furrow which is developed on the surface of the matter of the germ. Within this furrow the brain and spinal cord are at first represented by an elongated strip of nervous matter, which strip, as the furrow closes to form a tube, also becomes tubular, and incloses within it, as the hollow of the tube, the little canal which persists in the center of the spinal cord. The front part of this nervous tube, which soon exhibits a division into gray and white matter, now begins to expand so as to form three swellings named *vesicles*. From these vesicles the brain and its parts are formed. The foremost swelling soon produces the parts known as the optic lobes, and also the structures which are destined to form the hemispheres or halves of the brain itself. The middle swelling contributes to the formation of certain important structures of the brain; and finally the cerebellum or lesser brain, along with the upper part of the spinal cord and other structures, appear as the result of the full development of the hinder or third swelling. Nor must we neglect to note that at first the human brain is completely smooth and destitute of convolutions, and only acquires its convoluted appearance toward the completion of development.

It is now an appropriate duty to inquire if the history of the brain's growth affords any countenance or support to the phrenological division of the organ into some thirty-five different organs and seats of faculties. The query is further a perfectly legitimate one. The phrenolo-

gist maintains the actuality of his deductions respecting the "organs" of mind, and it is only a fair and just expectation that, if the brain be a congeries of such organs, the anatomist should be able to see these parts as development has revealed them. The nature of the brain is asserted by the phrenologist to exist in its composition as a set of organs. That nature, argues the anatomist, if revealed at all, should present itself in its development, which alone can show us nature's true fashion of building a brain. What, therefore, is the result of the anatomist's study of the manner in which the brain is fashioned? The answer is found in the statement that there is not a trace of a single "organ" such as the phrenologist theoretically maintains is represented in the brain. There is no division into separate parts and portions, as the phrenologist's chart would lead the observer to suppose. The scalpel of the anatomist can nowhere discover in the full-grown brain an organ of veneration, or of hope, or of language, or of destructiveness, or of any other mental feature: nor can his microscope detect in nature's wondrous process of fashioning the brain any reason for the belief that the organ of mind is a collection of parts each devoted to the exercise of a special quality of mind. The arrangement which appears so clear on the phrenologist's bust is nowhere represented in the brain itself. And the organs of the phrenologist, in so far as their existence is concerned, may not inaptly be described in Butler's words as being

> Such as take lodgings in a head
> That's to be let unfurnishèd.

But if development gives no support to the phrenological assertion of the brain's division into organs of the mind, neither does anatomy, human and comparative, countenance its tenets as applied to the examination of the brain-pan itself. To select a very plain method of testing the deductions of phrenology, let an anatomical plate of the upper surface of the undisturbed brain be exhibited, and, having settled the position of certain "organs" from a phrenological chart, let any one try to discover if the limits of any one organ can be discerned on the brain-surface. He will then clearly appreciate the hopeless nature of the task he has undertaken, and be ready to shrink from the attempt to resolve the complex convolutions before him into a square inch here of one faculty, or a square inch there of another. Moreover, one very important consideration will dawn upon the reflective mind which considers that the convolutions of the brain are not limited to the crown and sides of the head, but, on the contrary, extend over the entire surface of the cerebrum, and are developed on its base (see Fig. 2). No phrenologist has attempted, it is true, to get at the base of the brain by inspecting the palate; but it would be regarded as an absurd and unwarrantable statement to assert that the base of the brain has no functions, and that the mind of man is located only at the top and on the sides of the head. Yet the phrenologist is in the position of one

making such an assertion, since his science takes no account of the base or internal parts of the brain—situations, forsooth, in which anatomy and the newer phrenology demonstrate the existence of very important sensory and other organs. The question of the relatively im-

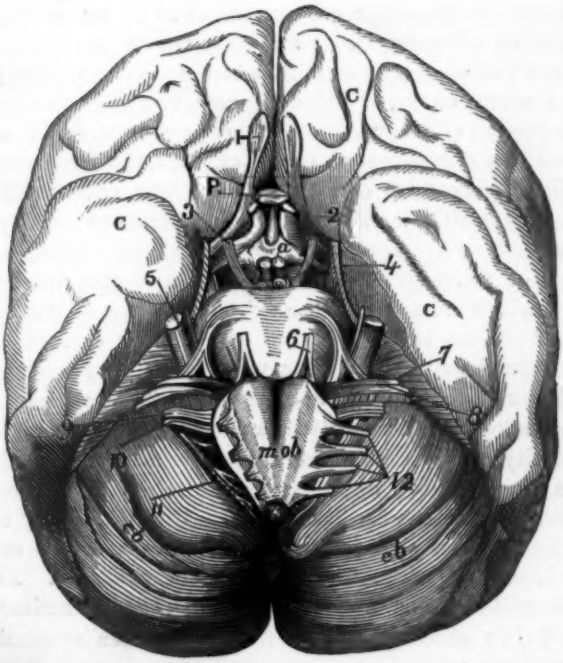


FIG. 2.—THE BASE OF THE BRAIN.—(From Bourgery.) *C*, under surface of the cerebrum; *cb*, the cerebellum; *m.ob*, the medulla oblongata. The nerves are numbered 1 to 12; 1, the olfactory nerve; 2, the optic; 3, 4, and 6, nerves which govern the muscles of the eyeball; 5, the trigeminal, which arises as shown by two roots; 7, the facial; 8, the auditory; 9, the glosso-pharyngeal; 10, the pneumogastric; 11, the spinal accessory; 12, the several roots of the hypoglossal. The figure 6 is placed on the pons varolii; the crura cerebri are between the third and fourth nerves on either side. Just above are *a*, the corpora albicanta, and *P*, the pituitary body.

mense tracts of brain which lie without the utmost ken of phrenology, even on its own showing, is also illustrated by the observation that the bulging or hollowing of the skull at any point affords no criterion of the thickness of the gray matter of the brain, a layer which we have already seen to constitute the most important part of the brain-substance. This gray matter is seen to exist in tolerable uniformity over large tracts of brain-substance, and it is invariably in the hinder region of the brain that it attains its greatest complexity and development. The form of the skull is dependent on the amount and disposition of the white matter, and not on that of the gray; and the former, as we have seen, has but a minor influence or part in the mental constitution, since its function is merely that of conducting and not of originating thoughts and impressions. Since, then, phrenology lays so much stress on skull-conformation as a clew to brain-structure, it must

be regarded as dealing rather with the results of the disposition of the white matter than with that of the gray—and this latter assumption of necessity involves a second, namely, that phrenology has no status as a science of mind at all.

There is one consideration concerning the practical application of the phrenologist's assertions too important to be overlooked, namely, the difficulty of detecting or of mapping out on the living head the various "bumps" or organs of mind which appear to be so lucidly localized on the bust or chart. The observer, who might naturally think the determination of the "bumps" an easy matter, has but to try to reconcile with a phrenological chart, or with the brain-surface itself (Fig. 1), the configuration of a friend's cranium, and he will then discover the impossibility of distinguishing where one faculty or organ ends and where another begins. How, for instance, can the exact limits of the four or five organs of mind, to be hereafter alluded to more specifically, which are supposed to exist in the line of the eyebrow, be determined? What is the criterion of excessive or inferior development here, and how may we know when one "encroaches" upon another to the exclusion or atrophy of the latter? The practical application of phrenology indeed constitutes one of its difficulties; and added to the difficulty or impossibility of accurately mapping out the boundaries of the phrenologist's organs, we must take into account the fact that we are expected to detail these organs through, in any case, a considerable thickness of scalp, which veils and occludes, as every anatomist knows, the intimate conformation of the skull-cap. At the most the phrenologist may distinguish regions; his exact examination of the living head *à la* phrenological chart or bust is an anatomical impossibility.

But the anatomist has also something of importance to say regarding the actual existence of certain of the "organs" of mind mapped out by the phrenologist. Leaning trustfully upon their empirical deductions, the phrenologists have frequently localized faculties and organs of mind upon bony surfaces separated from the brain by an intervening space of considerable kind. In so far as comparative anatomy is concerned, phrenology receives no assistance in its attempt to localize mind-functions in man. An elephant is admittedly a sagacious animal, with a brain worth studying; just as a cat or tiger presents us with a disposition in which, if brain-science is applicable, as it should be, to lower forms of life exhibiting special traits of character, destructiveness should be well represented and typically illustrated. Alas for phrenology! the bump of destructiveness in the feline races resolves itself into a mass of jaw-muscles, and the elephant's brain is placed certainly not within a foot or so of the most skillful of phrenological digits. The "frontal sinuses" or great air-spaces in the forehead bones of the animal intervene between the front of the brain, the region *par excellence* of intellect according to phrenology, and the outside

layer of the skull. So that an observer could no more accurately construct a phrenological chart of an elephant than he could diagnose the contents of a warehouse by scanning the exterior of the building.

Not merely, however, are the difficulties of phrenology limited to the lower animals. Suppose we make a cross-section of a human skull through either the right or left side of the forehead, about half an inch above the upper border of the orbit or eye cavity. We may then discover that man as well as the elephant possesses "frontal sinuses" or air-spaces in his forehead-bone of considerable extent intervening between the exterior of the skull and the contained brain. Now, in such a section of the human skull, what phrenological "organs" shall we cut through? Certainly those of "individuality," "form," "size," and "color." In placing such organs across the eyebrows, the phrenologist might naturally be regarded as having proceeded on the assumption that he was mapping out on the exterior of the skull a certain part of the brain-surface. What shall be said of his procedure, however, when the reader learns that a section of the skull made as indicated through these organs shows that they—i. e., the "organs" as marked on the outside of the skull—overlie the hollow spaces or "frontal sinuses," and are actually separated from the brain by cavities of considerable extent, in some cases exceeding an inch? Such a demonstration truly speaks for itself, and no less so does the anatomist's discovery that the "organ" of phrenologists known as "form" actually reposes in anything but a noble position on the cavity of the nose; that the organ of "calculation" is a solid bony (*orbital*) process; and that the size of the organ of "language" really depends upon the want of forward projection of the eye depending on the special development of a bony process on which the organ of sight rests, and which in any case has nothing whatever to do with the brain. Of language more anon; but enough has been said to show that a connection with the brain is not an invariable or apparently necessary condition for the construction of a phrenological "organ" of the mind—the fact that the brain is the organ of mind notwithstanding.

But neither does the case for phrenology fare any better when it is tested by the results of the examination of crania belonging to persons whose family or personal history was well known, and whose characters, in respect of their thorough and stable formation, would therefore serve as a test of phrenological or any other system of mind-explanation. In the heyday of phrenological discussion, and in Edinburgh as the very focus and center of the arguments *pro* and *con* the system of Gall and Spurzheim, a Mr. Stone, then President of the Royal Medical Society, read in 1829 a paper in which the results of a most laborious and conscientious series of observations on the crania of well-known persons were detailed. These results, as will presently be shown, were fatal to any ideas which might have been entertained regarding the authentic nature of the data on which phrenological observations were founded.

Fifty skulls were selected for measurement from the famous collection of Sir William Hamilton, fifty others being taken from that of Dr. Spurzheim himself. In the case of the skulls of fifteen murderers, whose crimes had been marked by unusual brutality and violence, and who might therefore be regarded as exemplifying cases in which the largeness of the "organ" of destructiveness might be lawfully postulated by a phrenologist, Mr. Stone demonstrated by careful measurement and comparison that each of the fifteen had the organ or surface of "destructiveness" *absolutely less* than the average of ordinary heads, while thirteen of these skulls possessed this organ *relatively less* when compared with the whole contents of the brain-pan. Nor was this all. Thirteen of these fifteen worthies possessed a larger organ of "benevolence" than the average, and their "conscientiousness" was also as a rule well developed. Their brains were not markedly deficient in front of the ear—the region of the intellectual faculties, according to the phrenologist—nor were they unusually developed behind the ear, where the animal faculties are supposed to reside.

No less instructive were the comparisons instituted between the faculties of Dr. David Gregory, once Professor of Mathematics in the University of Edinburgh, and Savilian Professor of Astronomy at Oxford, a friend and contemporary of Sir Isaac Newton. Professor Gregory's character was well known as that of an amiable, accomplished, intellectual man. In such a case the moral faculties would be expected to present high development, while the animal faculties and baser qualities would naturally be regarded as being but poorly represented. Mr. Stone's measurements, duly verified by independent observers, elicited the awkward fact that Dr. Gregory should, according to the phrenological interpretation of his cranium, have ranked in the criminal category, since his organ of "destructiveness" was found to exceed in size that of every murderer in the collection under discussion! In proportion to the general size and form of the brain, Dr. Gregory's "destructiveness" was larger than that of the notorious Burke, who was executed at Edinburgh for the cold-blooded murder of men, women, and children, whose bodies, along with his coadjutor Hare, he sold for purposes of anatomical inspection. Not to enumerate in detail the startling results which the fair and unbiased examination of Dr. Gregory's cranium afforded, it may simply be mentioned that the Professor's "combativeness" was larger than that of any of the debased villains with whom his faculties were compared. Burke equaled him in "benevolence"; in "secretiveness" he excelled the noteworthy fifteen; his "acquisitiveness" exceeded that of Haggart and other noted thieves; his "causality"—the power of reasoning closely, and of tracing the relations between cause and effect, a faculty which as a mathematician he should have possessed largely developed—was less than that of the criminals; and his intellectual faculties at large were of less capacity than theirs, as his animal faculties were present in greater force.

No further illustration is required of the fact that, tested under exceptionally favorable circumstances, the deductions of phrenology are absolutely incorrect, not to say absurd. Nor is the case of the phrenologists bettered by their exercise of apologetics in face of the hard logic of the above and similar facts. Thurtell, with very large "benevolence" and with well-developed "veneration," yet committed an atrocious murder, and this without a special development of "destructiveness." "Nothing can justify the murder," said the phrenologists, but Thurtell imagined that he would "do a service to society by killing his friend" (where his benevolence?) "and hence his crime." Thus benevolence, by the exercise of phrenological apologetics, becomes an excuse for and an active cause of murder. Dr. Gregory's "destructiveness," said the phrenologists, was held in check by some other qualities—by which qualities it would be hard to say, seeing that, tested by phrenology, his whole mental and moral organization was below that of the average murderer. So that we are to believe, in short, that "destructiveness," and the other base qualities of the Professor, being absolutely useless, must have been intended simply for show and not for use. Things, on this reasoning, truly are not what they seem; and phrenology thuswise cuts away from under itself its fundamental propositions, that its "organs" are the seats of faculties, and that their activity is proportional to their size.

But to proceed further would be to slay the slain. Thus much indeed we have said of the phrenology which still lingers in our midst, by way of contrast with the newer order of brain-interpretation which the advance of physiology has caused to arise among us. In the early days in which the battle of phrenology was fought and won as against the science of brain-pans, physiological experimentation upon the brain was an unknown and unworked source of information. In due time came Flourens, Magendie, Fritsch, Hitzig, and Ferrier, with their exact methods and results, enlarging the conceptions of the brain and its powers, and throwing here and there a ray of light upon the dark places and hidden corners in the domain of the physiology of mind. Hence our new "phrenology"—for the word itself is perfectly explicit as denoting a science of mind or brain—is gradually being built up from sure data and accurate experimentation, the results arrived at by one worker being tested by a host of fellow-experimenters ere his inferences become facts, and before they are allowed to form part and parcel of the scientific edifice. Let us briefly see what are the more prominent facts concerning the brain and its functions which recent science has elucidated.

No part of the brain has perhaps presented problems of such interesting character as the *cerebellum* or lesser brain which, as already remarked, exists at the hinder and lower part of the head, and which moreover presents us with a structure differing from that of the cerebrum itself. Phrenologists located in the cerebellum the purely animal

faculties. "A man," as we remember hearing a phrenological lecturer say, "with a head bulging out behind, is going backward in the world"; and there was indeed, as we shall see, a modicum of truth (although he knew and understood it not) in the lecturer's remark, since without the cerebellum we could in reality proceed neither forward nor backward. We now know that the old phrenology of the cerebellum is utterly wrong and unfounded. The new phrenology has shown us that in cases of diseased animal appetites, which in our lunatic asylums are but too frequently represented, the cerebellum is not found to be affected—a result explained by the fact that the appetites referred to are indeed as much part of our "mental" constitution as is the exercise of benevolence or of any other mental faculty. Furthermore, the new phrenology supplies positive evidence as to the true functions of the cerebellum. When it is removed from a pigeon, for instance, the animal retains its faculties, it will feed, it can see and hear, but is utterly unable to maintain its equilibrium. If thrown into the air, it flaps its wings in an erratic and aimless fashion. In one word, it can not "co-ordinate" its movements, that is, it can not so adjust the motions of one set of muscles as to bring them into purposive harmony with another set or series. The cerebellum thus appears to be the great brain-center whence are issued the commands and directions which guide the muscular actions and movements of our lives. Contrariwise, the true functions of the cerebellum are proved by experiments in which this part of the brain has been left intact while the cerebrum or true brain has been removed—an operation absolutely painless, as will presently be more particularly mentioned. A bird or higher animal in such a case will lose all power of volition; it will be deprived of sight, hearing, and other senses; it will die of hunger unless fed; it will exhibit no desire to move; and will in short present a condition utterly opposed to that seen when the cerebellum is removed and the true brain left intact. But with its cerebellum present, and *minus* its true brain, the bird can perfectly "coördinate" its movements. It will fly straight if thrown into the air, it will walk circumspectly enough if pushed forward, and will exhibit in fact such perfect muscular control, despite its want of volition and intellect, that the functions of the cerebellum as a controller of movements are no longer matter of hypothesis, but have become stable physiological fact.

If, however, the old phrenology has been displaced from the cerebellum by the new, no less important is it to note that, regarding the functions of the true brain, modern research has been equally successful in deposing the old ideas of the "organs" and their attendant faculties as exhibited on the phrenological charts and busts. Experimentation on the brain of higher animals, *quoad* the brain itself, is absolutely painless—contrary to popular notions and ideas. True, there are certain parts of the brain which are exceedingly delicate, and in which the point of a needle would inflict at once a fatal injury. But the

brain-substance itself is utterly non-sensitive, as every hospital-surgeon can tell us. Persons may actually recover from serious injuries of the brain in which several ounces of brain-substance may have been lost, and recover with good effect, and in many cases without any perceptible alteration of their mental peculiarity. The most notorious case of this kind is known as "the American crow-bar case." A bar of iron accidentally shot off from a blast passed through the top of a young man's head at the left side of the forehead, having traversed the front part of the left hemisphere or side of the brain. The iron bar measured three feet in length, and weighed fourteen pounds. After the accident he felt no pain, and was able to walk without help in a few hours' time. The man made a good recovery, and for twelve years made a livelihood by exhibiting himself in the United States, his skull being now preserved in the museum of Harvard University. This patient undoubtedly lost a relatively large portion of his brain-substance. At one fell swoop there must have been a considerable destruction of phrenological organs. Yet he suffered from no deprivation of intelligence; and few would dream of associating the drinking habits which finally beset him with his accident and with his loss of brains, or otherwise maintain that he was less rational before than after the accident. Thus the misfortunes of existence and the experimentation of the physiologist positively contradict the old phrenology, and assert that localization of function does exist, it is true, but that the "organs" of the phrenologist are mere theoretical nonentities, without a trace of substance to insure their stability or real nature.

What amount of localization, then, can be safely assumed to exist in the human brain as revealed by recent experimentation? It may be known to the generality of readers that the movements, acts, and probably ideas relating to one side of the body are regulated by the opposite side or hemisphere of the brain. Thus, convulsions affecting one side of the body were shown by Dr. Hughlings Jackson to be caused by disease of the opposite side, and the idea of the duality of the brain's action followed in a natural sequence on the observation of facts like the preceding. Thus, as a general rule, it may be affirmed that brain-disease itself, or the ideas of natural existence, are so far localized that their perfect effects are only visible and appreciated when the same parts in both halves or hemispheres of the brain are affected. To illustrate what the new phrenology has to say regarding the localization of the brain-functions, let us inquire what is known regarding the exceedingly curious condition known as "aphasia." Persons affected with this lesion understand perfectly what is said to them, but they are absolutely speechless, and can not utter a single word. Now, it is a perfectly well-ascertained fact that aphasia is associated with disease of the front part of the *left* half or hemisphere of the brain—a part which may therefore be called the "speech-center." The curious fact must thus be emphasized that aphasia is invariably associated with disease

of the left, and never with disease of the right side of the brain. To the brief explanation of this curious fact we shall presently return; but we may in conclusion remark certain facts now known respecting the localization of other functions. Professor Ferrier, of King's College, London, employing electricity as the only agent and means of stimulation to which the non-sensitive brain will respond, has succeeded in mapping out in the brains of higher animals the centers which govern many of the common movements of life, and which from reasonable analogy may be presumed to be represented in the human brain as well. As these acts are the practical outcome of ideas, the parts of the brain concerned in the production of definite ideas may thus be regarded as being in one sense mapped out and recognized; although it is hardly necessary to remark that the regions of Dr. Ferrier in no wise correspond to those of the old phrenology, while in many cases, indeed, they are utterly opposed to it. Thus the sense of touch is found to be localized in the inner surface of the hemispheres of the brain, and this fact alone tells against the phrenologist, to whom the mere brain-surface is the brain itself.

Thus the work of localizing movements and important centers of the senses has so far proceeded with success. There yet remains for observation the curious case of aphasia or speechlessness, and its location in a "speech-center" or "speech-organ" in the front of the left hemisphere of the brain. It is a noteworthy fact in brain-physiology, that when an animal has been rendered blind by the destruction of the sight-center of one side, blindness disappears and sight gradually returns, since the remaining and normal sight-center of the opposite side assumes the functions of its neighbor. Complete blindness only ensues when both sight-centers are diseased. The same remark holds good of the movements of the mouth and tongue in speech, these being "bilateral," so that the center of these latter movements on one side may be destroyed without causing paralysis of the tongue, provided the center of the other side is uninjured. Movements of the hands and feet are, on the contrary, one-sided. Destruction of one center governing these latter movements insures complete cessation of the movements on the opposite side of the body. Now, in aphasia or speechlessness, we merely perceive the results of the destruction of the single speech-center—the left—which man normally possesses. Just as we use the right hand in preference to the left in prehension and in writing, and as the movements of this hand are regulated by the left side of the brain, so our faculty of articulation is also unilateral and single-handed, so to speak. The memory of sounds and words forms the basis of our speech —"the memory of words is only the memory of certain articulations" —and those parts of the brain which regulate articulation are also the memory-centers for speech or the result of articulation. Thus, when the speech-center is disorganized, not merely the power of articulation disappears, but also the memory of words. But while the left side is

that of the speech-center, there is no reason, as Dr. Ferrier remarks, apart from heredity and education, why this should necessarily be so. "It is quite conceivable," says this author, "that a person who has become aphasic by reason of total and permanent destruction of the left speech-center may reacquire the faculty of speech by education of the right articulatory centers." We speak with the left side of our brains, in short, not because we are unable to do so with the right side, but simply because habit and the law of likeness together strengthen and perpetuate the custom of speaking with the left. But it may be also supposed that, as a left-handed person must regulate the movements of his arms chiefly by the right side of his brain, so there may exist subjects who naturally use the right instead of the left speech-center.

Whatever results may in future accrue to human knowledge from researches into the functions of the brain, no one may doubt the all-important nature of the knowledge which literally enables man to know himself, and to understand in some degree the mainsprings of the actions which constitute his daily existence. The subject is also no less instructive in the sense in which it shows the displacement of erroneous ideas by new and higher thoughts founded on accurate observation of the facts of life; while in a very direct fashion such higher knowledge may affect suffering humanity, since an educated medical science, furnished with secure data regarding the causes of mental affections, may successfully "minister to minds diseased," and even in due time raze out the troubles which perplex many a weary soul.—*Gentleman's Magazine.*

BACKGAMMON AMONG THE AZTECS.

By EDWARD B. TYLOR.

BY *backgammon* we usually mean one particular game played with dice and thirty draughts, on a board with twelve points on each side. But this is only one of a family of games, whose general definition is that they consist in moving pieces on a diagram, not at the player's free choice, as in draught-playing, but conformably to the throws of lots or dice. It can hardly be doubted that the set of games thus combining chance and skill are all, whether ancient or modern, the descendants of one original game. By a stretch of imagination, it may be possible to fancy draughts or dice to have been fresh invented more than once. But, when it comes to a game which combines the two ideas, it seems to pass the bounds of ordinary probability to suppose, for instance, that a Greek and an Arab and a Birmese were separately seized by the same happy thought, and said, "Go to, let us cast lots, and count them to play at draughts by." If indeed any reader should think such a combination might have happened twice over, he

may be asked to look closely into the games presently to be described, so as to satisfy himself that their agreement goes even further, as in the peculiar principle on which the high and low throws are counted, and, so far as one knows, in there generally being in some shape the rule of hitting a blot, that is, taking an enemy's undefended man off the point one's own man moves to. The exact primitive game whence all known games of the class were derived can not now be pointed out, and indeed is perhaps lost in prehistoric antiquity. So we may as well keep to our own word, and call the whole set the backgammon family. It is in this sense that I use the word here, with the purpose of proving that, before Hernando Cortes landed with his invading Spaniards at Vera Cruz, one variety of backgammon had already found its way over from Asia into Mexico, and had become a fashionable amusement at the barbaric court of Montezuma. But, before following the game on its hitherto unnoticed migration into the New World, let us first glance at its Old World history.

Clearly our English *backgammon* and the more complicated French *trictrac* are descended from the Roman game of the "twelve lines" (*duodecim scripta*), which was played throughout the empire. This is the game which Ovid says has lines as many as the gliding year has months, and he means it where he gives the lover insidious counsel, when his mistress casts the ivory numbers from her hand, let him give himself bad throws and play them ill. Among the Christian antiquities in Rome is a marble slab, on which a backgammon-table is cut, with a Greek cross in the middle, and a Greek inscription that Jesus Christ gives victory and help to dicers if they write his name when they throw the dice—Amen. Carelessly scratched as it is, by some stone-cutter whose faith went beyond his trictrac, it shows that the board was like ours even to the division in the middle, which makes the two groups of six points on each side. From ancient Rome, too, we inherit the habit of making the backgammon-board with a draught-board on the reverse side, at any rate the commentators so interpret Martial's epigram on the *tabula lusoria:*

Hic mihi bis seno numeratur tessera puncto
Calculus hic gemino discolor hoste perit.

Here, twice the die is counted to the point of *size,*
Here, 'twixt twin foes of other hue, the draughtsman dies.

The very mode of playing the men in classic backgammon may be made out from a fifth-century Greek epigram, commemorating a remarkable hit, in which the Emperor Zeno got his men so blocked that, having the ill-luck to throw 2, 5, 6 (they used three dice, as indeed we continued to do in the middle ages), the only moves open obliged him to leave eight blots. This historic problem, and other matters of Greek and Latin backgammon, are worked out by M. Becq de Fouquières, in his "Jeux des Anciens," with a skill that would have rejoiced the hearts

of those eminent amateurs, the old Count de Trictrac and the venerable Abbé du Cornet, to whose teaching history records that Miss Becky Sharp ascribed the proficiency at backgammon which made her society so agreeable to Sir Pitt at Queen's Crawley.

It is not known so exactly what manner of backgammon the Greeks played in earlier ages; but there are various passages to prove that, when they talk of dice-playing, they often mean not mere hazard, but some game of the backgammon sort, where the throws of the dice are turned to account by skillful moving of pieces. Thus Plato says that, as in casting dice, we ought to arrange our affairs according to the throws we get, as reason shall declare best; and Plutarch, further moralizing, remarks that Plato compares life to dicing (*κυβεία*), where one must not only get good throws, but know how to use them skillfully when one has got them. So with Plutarch's story of Parysatis, mother of Artaxerxes. She was "awful at dice" (*δεινὴ κυβεύειν*), and, "playing her game carefully," won from the king the eunuch Mesabates, who had cut off the head and hand of Cyrus; having got him, she had him flayed alive and his skin stretched. This episode of old Persian history is noteworthy in the history of the game, because Persian backgammon, which they call *nard*, is much like the European form of the game, which, it has not been unreasonably guessed, may itself have come from Persia. This nard is popular in the East, and orthodox Moslems have seen in the fateful throws of the dice a recognition of the decrees of Allah, that fall sometimes for a man and sometimes against him. It is, said one, a nobler game than chess, for the backgammon-player acknowledges predestination and the divine will, but the chess-player denies them like a dissenter. Not to lose ourselves in speculations on the Oriental origin of backgammon, at any rate it was from Rome that it spread over Europe, carrying its Latin name of *tabulæ* with it in French and English *tables*. This word has dropped out of our use since the Elizabethan period, but an instance of it may be cited in a couple of lines, conveying another little sermon on backgammon, which the English author no doubt borrowed from the Latin of Terence, even as he had copied it from the Greek of Menander:

> Man's life's a game of *tables*, and he may
> Mend his bad fortune by his wiser play.

There is an idea which readily presents itself as to how backgammon came to be invented, namely, that the draughts were originally mere *counters*, such as little stones, shifted on a calculating board to reckon up the successive throws, and that it was an afterthought to allow skill in the choice of moves. This guess fits well enough with the classic draught being described as a stone, *ψῆφος*, *calx* or *calculus*, while in Germany, though now made of wood, it still keeps its old name of *stein*. Also the playing board on which the stones were moved shares the name of the calculating board, *ἄβαξ*, *abacus*. But if the classical

varieties of backgammon in this way show traces of the game near its original state, they seem in another respect to have passed out of their early simplicity. They are all played with dice, and indeed the French author lately mentioned seems right in guessing that the division of our board into groups of six points each was made on purpose to suit the throws of cubical dice like ours, numbered on all the sides, from 1 to 6. As to the early history of dice, I have elsewhere endeavored to show ("Primitive Culture," chapter iii.) that the origin of games of chance may be fairly looked for in instruments of the nature of lots, at first cast seriously by diviners for omens, and afterward brought down from serious magic into mere sport. Now, the simplest of such instruments is the lot which only falls two ways, like the shell, white on one side and blackened on the other, which Greek children spun up into the air to fall, "night or day," as they said; or, like our half-pence, tossed for "head or tail." Both in divination and in gambling, such two-faced lots probably came earlier than the highly artificial numbered dice. The kinds of backgammon now to be described seem in general to belong to the earlier stage of development, for it is with lots, not dice, that they are played.

The traveler in Egypt or Palestine now and then comes on a lively group sitting round a game, and in their eager shouts, if he knows some Arabic, he may distinguish not only such words as "two" or "four," but also "child," "dog," "Christian," "Moslem. On closer examination he finds that the game is called *táb*, and that it is a sort of backgammon played on an oblong checker-board, or four rows of little holes in the ground, where bits of stone on one side and bits of red brick on the other do duty as draughts, being shifted from place to place in the rows of squares or holes. Not dice, but lots, are cast to regulate the moves; these lots are generally four slips of palm-stick, with a green outer side and a white cut side (called black and white), and when they are thrown against a stick set up in the ground, the throw counts according to how many white sides come up, thus:

Whites up:	None	One	Two	Three	Four.
Count:	6	1	2	3	4
	(go on)	(táb)	(stop)	(stop)	(go on)

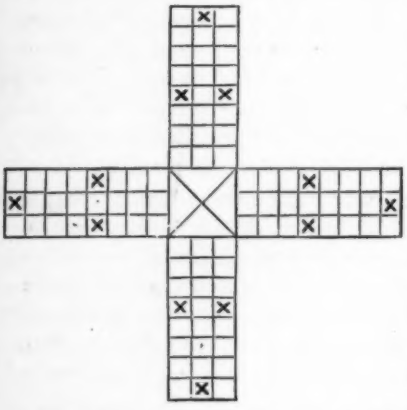
Notice particularly this way of counting throws, for its principles will be found again in lot backgammon elsewhere. There is evidently a crude attempt to reckon probabilities, giving a higher value to the less frequent throws of all four white and all four black, than to two or three white, which come up oftener. Besides the high count, they have the privilege of a second throw. This, if lot backgammon came first, and was succeeded by dice backgammon, would naturally pass into our rule of giving doubles another throw. The throw of one white, which is called "child," or *táb*, i. e., "game," has a special power, for only by it may a "dog," that is, a stone or draught, be moved out of its original place in the outer row, and set at liberty to circulate along the lines of

squares or "houses," taking an enemy's dog if found alone in its house. While a draught is still in its first inactive, useless condition, they call it a "Nazarene," or Christian; but, when the throw of *táb* gives it the right to go forth conquering and to conquer, it becomes a "Moslem." It is not needful to go further into the rather complicated rules of moving and taking. Those who are curious may find much about it in Lane's "Modern Egyptians," and in the quaintly learned little book "De Ludis Orientalibus," by Thomas Hyde, who was Bodleian librarian in the reign of William and Mary. But one question suggests itself. Seeing how the modern fellahs delight in *táb*, one naturally asks, Did they inherit it from the ancient Egyptians? From remote antiquity the Egyptians played draughts on earth, and after death their righteous souls still had the oblong checker-board, and the men like chess-pawns, to amuse their glorified but perhaps rather tiresome life in the world below. But, as Dr. Birch points out, no Egyptian dice have been found earlier than Roman times, nor any plain mention of backgammon. Even if they played like their descendants in the Nile Valley with such things as slips of palm, something about it should be found in the hieroglyphic texts. But at present nothing appears, and there is no reason to add backgammon to the long list of inventions whose earliest traces are found in Egypt. Perhaps the nearest relative of *táb* is Chinese backgammon, but this is played with dice.

Next, as to India. Here, since ancient times, cowry-shells have been thrown as lots, their "head" and "tail" being according as the shell falls with mouth or back upward. In Sanskrit literature there is an old mention of a game called *panchikâ*, which was played with five cowries, and where it seems that the winning throws were when all the mouths came up or down, as against the commoner throws when some fell each way. That a game of the nature of backgammon was known in India from high antiquity has been plainly made out by Professor Weber. It was called *ayânaya*, or "luck and unluck"; or at any rate that was a term used as to the moving of the pieces, which traveled right and left through the squares, and took an undefended man from his place to begin his course anew. So, as a Sanskrit riddle has it: "In a house where there were many, there is left but one, and where there was none and many come, at last there is none. Thus Kâla and Kâlî, casting day and night for their pair of dice, play with human beings for pieces on the board of the world." Putting these particulars together, it is clearly possible to trace from ancient times the game of *pachisi*, played in modern India, into which game it will now be necessary for our argument to go more exactly; in fact, to qualify ourselves to sit down and play a game. English backgammon-players will hardly take five minutes to learn it.

Suppose four players to be seated, each at the end of one arm of the diagram or board, of which a figure is here given. Each player will have four little wooden cones as his pieces or draughts, all of one

color, to distinguish them. If only two play, each will manœuvre two sets of men. Each player's men start one by one down the middle row of his own rectangle, beginning with the square next the central space, and thence they proceed all round the outside rows of the board, traveling from right to left (contrary to the sun) till they get back to their own central row, and up it home to where they started from, he who first gets all his men home winning the game. A solitary man is taken up and sent back to begin again, by one of his adversary's men lighting upon his square, except in the case of the twelve privileged squares, which are marked with a cross, in which case the overtaking piece can not move. The moving is determined by throwing a number of cowries, which count according to how many fall mouth up ; thus, if six cowries are used :



Mouths up:	None	One	Two	Three	Four	Five	Six.
Count:	6	10	2	3	4	25	12
	(go on)	(das)	(stop)	(stop)	(stop)	(pachisi)	(go on)

According to the rules kindly sent me from Dr. Rajendralala Mitra, of Calcutta, the throws of one-up or five-up (*das* or *pachisi*) alone can start a man on his first square, or get him off if stuck on his last square. These throws, as well as none-up and six-up, give a new throw. Thus the best beginnings are one-up followed by two-up, or five-up followed by four-up, either of which enters a man and carries him on in safety into a "fort." Seven cowries can also be used, but the primitive game was probably more like the ancient game with five cowries just mentioned, for the name pachisi means "five-and-twenty," and was no doubt taken from the throw when five shells come up. The principles of counting the throws and entering the men are plainly like those in the Arab game of táb, and there are Indian forms with only four cowries which come still closer.

Pachisi is a favorite game in India, and an eager player will carry rolled round in his turban the cloth which serves as a board, so as to be ready for a game at any moment. These cloths, when embroidered with the diagram in colored silk, are quite artistic objects, and one does not often see prettier toys than a set of men in Mr. Franks's collection, little cones (or rather sugar-loaves) of rock crystal, with the colors they are to bear in the game shown by mounting in the top a ruby for red, an emerald for green, etc. There are even stories of yet more

sumptuous games, where the board was a courtyard laid out in marble pavement, on which living draught-men clothed in green, red, yellow, and black, walked the circuit and hustled one another off the squares. Our Anglo-Indians sometimes catch the enthusiasm; and there is an often-told tale of that official personage who, when he paid his native servants their wages, would sit down with them to a match at pachisi, and sometimes win his money back. In London toy-shops they sell board and pieces for what they profess to be the game, but these really belong to the modified form of it known in India as *chûpur*, in which, instead of cowries, stick-dice numbered on the four long sides are thrown, these Indian dice being in England replaced by our common cubical ones. This shows the change from lots to dice in games of the backgammon sort, and it is curious to notice how clearly the new rules for counting by the dice are modeled on the old rules for throws of cowries. Having now sufficiently mastered the peculiarities of pachisi, let us pass from Asia to America, and compare them with the details of the Mexican game of *patolli*.

When the Spanish invaders of Mexico gazed half in admiration and half in contempt on the barbaric arts and fashions of Aztec life, they particularly noticed a game, at which the natives played so eagerly that, when they lost all they had, they would even stake their own bodies, and gamble themselves into slavery, just as Tacitus says the old Germans used to do. The earliest particulars of the Mexican game come from Lopez de Gomara, whose "Istoria de las Indias" was printed in 1552, so that it must have been written while the memory of the conquest in 1521 was still fresh. He says: "Sometimes Montezuma looked on as they played at *patoliztli*, which is much like the game of tables, and is played with beans marked with lines like one-faced dice, which they call *patolli*. These they take between both hands, and throw them on a mat or on the ground, where there are certain lines like a checker-board, on which they mark with stones the point which came up, taking off or putting on a little stone." This may be supplemented from three other old Spanish writers—Torquemada, Sahagun, and Duran. The figure on the mat is spoken of as "a painted cross full of squares like checkers," or as an "aspa," which word means a +, a Greek cross, the sails of a windmill, etc., descriptions which come as close as may be to the pachisi-board. Also, it appears that the stones moved on the board to mark the numbers thrown by the beans were of different colors, one account mentioning twelve stones, six red and six blue, between the two players.

According as the game was played, three to five beans were thrown as lots or dice, and sometimes these beans were marked on one side with a hole, and left plain on the other, while sometimes they seem to have had dots or lines indicating various numbers. If both ways were really used, then the game was known in both its stages, that of two-faced lots and that of numbered dice, just as in India it is played as

pachisi with cowries, and as chûpur with stick-dice. As to the way of scoring the throws, only one of the old writers says anything. This is Diego Duran, an extract from whose MS. history I have obtained by the courtesy of Mr. Oak, of the Bancroft Library at San Francisco. He says, as to the holes in the beans which showed how many squares were to be gained, that they were "if one, one, and if two, two, and if three, three, but marking five they were ten, and if ten, twenty." Thus in Mexico we just catch sight of the peculiar trick of scoring, everywhere so characteristic of the game, namely, the advantage given to the extreme throws, which in our own backgammon takes the form of allowing doubles to count twice over. Unluckily, the thought had never crossed the minds of these early Spanish historians of the New World that their descriptions of the Aztec game would ever become evidence of use in tracing the lines along which civilization spread over the earth. Had they seen this they would have left us a perfect set of rules, not such careless mentions of a game which plainly they "did not understand." Still they saw enough of Montezuma's patolli to observe that it was in principle like their own game of tables, while clearly they had never heard of the Indian pachisi, or they would have seen how much closer its resemblance came to that. This touches a point in the history of the game. How did the Mexicans get it? The idea may have already occurred to some readers of this essay, Could not perhaps some stray Portuguese or Spaniard, having lately picked up the game of pachisi in some seaport of the East Indies, have taken his next voyage to the West Indies, and naturalized his newly-learned game on the mainland of America? But there is no room for a suggestion of this sort when it is remembered that patolli was an established diversion in Mexico at the time of the Spanish entry, which followed within three years of the first landing of Grijalva in the gulf of Mexico, and indeed within five-and-twenty years of Colon's first sight of Hispaniola. What seems most likely is, that the game came direct from Asia to America, reaching Mexico from the Pacific coast.

That the remarkable civilization of Mexico as the Spaniards found it was not entirely of native American growth, but had taken up ideas from Asia, is no new opinion. Alexander von Humboldt argued years ago that the Mexicans did and believed things which were at once so fanciful and so like the fancies of Asiatics that there must have been communication. Would two nations, he asks in effect, have taken independently to forming calendars of days and years by repeating and combining cycles of animals such as tiger, dog, ape, hare; would they have developed independently similar astrological fancies about these signs governing the periods they began, and being influential each over a particular limb or organ of men's bodies, would they, again, have evolved separately out of their consciousness the myth of the world and its inhabitants having at the end of several successive periods been destroyed by elemental catastrophes? In spite of Humboldt we often

hear Mexican culture talked of as self-produced, with its bronze and gold work, its elaborate architecture and sculpture, its monastic and priestly institutions, its complicated religious rites and formulas. It was my fortune years ago to travel in Mexico and explore its wonderful ruins, and ever since I have held to the view that the higher art and life of the whole Central American district is most rationally accounted for by a carrying across of culture from Asia. Thus it is now a peculiar pleasure to me to supplement Humboldt's group of arguments with a new one which goes on all-fours with them. It may very well have been the same agency which transported to Mexico the art of bronze-making, the computation of time by periods of dogs and apes, the casting of nativities, and the playing of backgammon. What that agency was one can as yet do no more than guess, but too much stress must not be laid on it in speculating on the mass migrations of the American races. Such matters as arts or games are easily carried from country to country; nor can we treat as inaccessible to Asiatic influences the Pacific coast of North America, where disabled junks brought across by the ocean current are from time to time drifted ashore, now and then with their crews alive. The Asiatic communication to be traced in the culture of the Aztec nation may not have been very ancient or extensive; all we can argue is, that communication of some sort there was.

Now one thing leads to another, especially in ethnology. Curiously enough, by following up the traces of this trivial little game, we get an unexpected glimpse into the history of the ruder North American tribes. Having learned about patolli as played in old Mexico, let us take up the account of a Jesuit missionary, Father Joseph Ochs, who was in Spanish America in 1754–'68, and who is here writing about the tribes of Sonora and Chihuahua: "Instead of our cards they have slips of reed or bits of wood a thumb wide and near a span long, on which, as on a tally, different strokes are cut and stained black. These they hold fast in the hand, lift them up as high as they can, and let them drop on the ground. Whichever then has most strokes or eyes for him wins the stake. This game is as bad as the notorious hazard. They call it *patole*. As it is forbidden on pain of blows, they choose for it a place in the bush; but the clatter of these bits of wood has discovered me many a hidden gamester. To play more safely, they would spread a cloak or carpet so as not to be betrayed by the noise." Here, then, is found, toward a thousand miles northwest of the city of Mexico, a game which may be described as patolli without the counters, and which still bears the Aztec name, in a district whose language is not Aztec, so that the proof of its having traveled from Mexico seems complete. The people, being less intellectual than the old Mexicans, have dropped the skillful part of the game and are content with the mere dicing. Nor, by the way, is this the only place where backgammon has so come down, for in Egypt they will lay aside the board and throw the tâb-sticks for fun, those who throw four and six being proclaimed Sultan and Vizier,

while the luckless thrower of two gets for his reward two cuts with the palm-stick on the soles of his feet.

Yet another fifteen hundred miles or more up into the continent the game is still to be traced. Among the hunting tribes known under the common name of the North American Indians, there is a favorite sport described by a score of writers under the name of "game of the bowl," or "game of plum-stones." The lots used are a number of plum-stones burned on one side to blacken them, or any similar double-convex pieces of wood, horn, etc. They are either thrown by hand or shaken in a bowl or dish, whence they can be neatly jerked up and let fall on the blanket spread to play on. The counting depends upon how many come up of either color, white or black, as is seen in the precise rules given by Mr. Morgan in his "League of the Iroquois." Where six "peach-stones" were thrown, if all six came up, white or black, they counted five, and five up, white or black, counted one, these high throws also giving the player a new turn, but all lower throws counted nothing and passed the lead. It is so curious to find the principle of lot-scoring, which we have tracked all across from Egypt, cropping up so perfectly among the Iroquois, that at the risk of being tedious it is worth while to give in full the mode of counting in the game as played with eight "deer-buttons." The following top line shows how many black or white sides up, with their count below:

Eight	Seven	Six	Five	Four	Three	Two	One	None
20	4	2	0	0	0	2	4	20
go on			stop			go on		

In these games there is no board to play on. The Iroquois use beans as counters, the game being won by one player getting all the beans, but perhaps the white men taught them how to do this. So with the game which will occur to English readers who remember it in "Hiawatha," where it is described at full length in prose-poetry as "the game of bowl and counters, *pugasaing* with thirteen pieces." This game is real enough; indeed, the description of it is taken from Schoolcraft's "Indian Tribes." But there seem to be no early mentions of this Algonquin game with its ducks and war-clubs and elaborate counting, nor of the Dakota game with tortoises and war-eagle son the plum-stones. Thus both may have been lately devised by Indians under European teaching, as improvements on the original *pugasaing* or "play," which was the simple game with black- and white-sided plum-stones, or the like. This, no doubt, is old, for it is described by the Jesuit missionaries in 1636 under the name of *jeu de plat*, as a regular sport among the Hurons; and as they clearly did not learn the game from Europe, we are left to argue that it reached them from Asia, very likely through Mexico.

It remains to glance at what may be learned as to the history of the North American Indians from the fact of their gambling with the bowl

and plum-stones. It is an interesting question whether "the poor Indian, whose untutored mind" has now and then been too easily credited with the invention of all the arts and beliefs he did not get from the white men, may not really before this have largely taken up in his culture ideas of Old-World growth. It has long been noticed that, looking at the native tribes of what is now the United States and the Dominion of Canada, the tribes on the east side had taken to making pottery and cultivating maize, while the tribes on the west had not, which seems as though there had been a flow or drift of civilization from the Central American district up the eastern half of the continent, which of itself ought to be enough to prevent any ethnologists from looking at the so-called red-man of New England or the Lakes as the creator of his whole industrial and social life. Nor is it an unknown thing that the myth and religion of the North American tribes contain many fancies well known to Asia, which the men of the prairies were hardly likely to have hit upon independently, but which they certainly did not learn from the white men, who did not even know them. If we are bound, as I think we are, to open a theoretical road for even a well-marked game to migrate by from Asia into America, then there are plenty of other matters waiting for passage along the route. By such conveyance of ideas it may be easiest to explain why the so-called Indians of North America shared with the real Indians of India the quaint belief that the world is a monstrous tortoise floating on the waters, or why the Sioux Indians share with the Tartars the idea that it is sinful to chop or poke with a sharp instrument the burning logs on the fire. But these considerations lead too far into the deepest-lying problems of the connection and intercourse of nations to be here pursued further. It is remarkable, too, how vast a geographical range the argument on the migrations of a game may cover. The American farmer now whiles away the winter evening in his farmhouse parlor with a hit at backgammon, on the spot where, not long since, the Iroquois played peach-stones in his bark hut. Neither would have recognized the other's sport as akin to his own, though when we trace them through the intermediate stages they are seen to be both birds of one nest. It is by strangely different routes that they have at last come together from their Asiatic home—one perhaps eastward through Asia, across the Pacific, into Mexico, and northward to the St. Lawrence; the other, no doubt, westward down to the Mediterranean, up northward to England, over the Atlantic, and so out into the American prairie.[1]—*Macmillan's Magazine.*

[1] For special details, copies of original documents, etc., see a paper by the author "On the Game of Patolli in Ancient Mexico, and its probably Asiatic Origin," read before the Anthropological Institute on April 9, 1878.

MITES, TICKS, AND OTHER ACARI.

By E. R. LELAND.

THE acari constitute a large order of minute animals, including mites, ticks, itch insects, etc., with which, in some of their forms, every one is more or less familiar, though, owing to their small size and obscure ways of living, but little is known of their structure and habits. They are often spoken of as insects, but are by scientific classification separated from the true insects, the most marked distinction being the possession of eight legs instead of six. Usually they are furnished with a suctorial apparatus. They are parasitic on both the animal and vegetable kingdoms. No class of animals is free from them. They prey upon each other; insects are infested with and support them at the cost of their lives; they attach themselves to fish and cold-blooded reptiles, and colonize on the whole range of birds and mammals. Some live under the skin, burrowing in the muscles; others dig corridors beneath the epidermis; while others, again, wander only on the surface. They are found in the lungs, air-passages, and intestinal canals of vertebrates. They are very prolific, and, though small, awkward, and slow-moving, they are transferred in a surprising way, often suddenly appearing about houses in enormous numbers, infesting plants and domesticated animals, or swarming on articles of food. Sometimes, especially in the tropics, they inflict considerable suffering; but it is probable that, on the whole, they are beneficial to man. A few directly injure him, and none directly benefit him, but indirectly they do him service by preying, as will be seen, on insect-pests and by acting as scavengers. The number of species in this family is so great as to forbid any detailed description, or even the merest mention of all of them. For the most part they are seldom seen except by those who make them a study. But a brief account of what is known about those of them that come into relations with man closely enough to affect his comfort and welfare, may be of interest and value.

The species thus referred to are found in the following classes: 1. Spinning and harvest mites, red spiders, etc. (*Trombidiinæ*); 2. Insect mite-parasites, sometimes called ticks (*Gamasidæ*); 3. True ticks (*Ixodidæ*); 4. Cheese and sugar mites, itch mites, etc. (*Acaridæ*)—the portion of the family commonly included under the name acari.

The spinning mites, like their relatives the spiders, can spin webs. They are of small size and semi-transparent. The most common is the red mite (Fig. 1), *Tetranychus telarius*, so well known to all who have the care of house plants. It is one twentieth of an inch long, yellowish, with two red spots on the sides, though its color

varies with the plant on which it is found, and in different individuals on the same plant; a rusty color shows maturity. This mite, like most of the genus, spins a web on the back of the leaves so delicate that a single strand can not be seen even with the aid of a common magnifier. When the web-work is done, however, it is plainly visible,

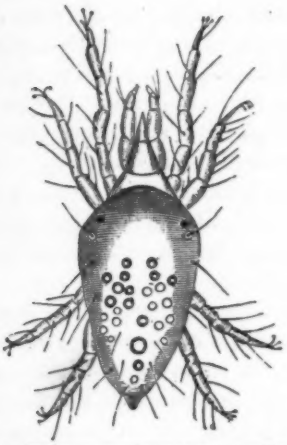


FIG. 1.—TETRANYCHUS TELARIUS—perfect insect, male.

FIG. 2.—TROMBIDIUM PARASITICUM.

FIG. 3.—TROMBIDIUM SERICEUM.

and under it the mites congregate in numbers, and feed and multiply very fast. The leaves soon look sickly, are marbled with gray and yellow above, the underside being white and shiny, with recurved edges. A microscope will show hundreds of mites of all ages, together with unhatched eggs pasted to the web; they are draining the leaf-vessels of their sap, and choking the breathing-pores with excrement. The remedies used to get rid of them are various, but sulphur seems the most potent of them all. It is recommended to lay flowers of sulphur on the heating-pipes in the hot-house, or it may be mixed with soapsuds, and applied to the leaves. Plain soap and water is effectual if made to reach the insect, but a bent syringe or some other means must be employed to reach the underside of the leaves, otherwise the mites will remain in comfort and safety through a hard drenching. Luckily for the florist, they are preyed upon by other mites and insects—the grub or larvæ devouring whole colonies of them very quickly. The cucumber, cacti, vine, etc., as well as the shrubs and herbage of wood and field, support various species of this genus; they are all of similar habits while confined to vegetation, and to be got rid of by similar methods. But some of them transfer themselves to the skin with seeming ease, and when thus lodged cause great discomfort, producing itching and redness, and sometimes eruptions similar to those of the itch. Two species known in the Mississippi Valley as jiggers are very annoying to those who have to expose themselves where they

abound. Prof. Riley names and describes them as follows: *T. Americanus*, a mite barely visible to the naked eye, moves readily, and is found more frequently upon children than upon adults. It lives mostly upon the scalp and under the armpits, but is frequently found on other parts of the body. It does not bury itself in the flesh, but works its head under the skin, causing irritation, followed by a red pimple. *T. irritans*, the best known and most troublesome of the two, causes intense irritation and swelling on all parts of the body, but more especially upon the legs and around the ankles. By the aid of its strong jaws, the lower pair of which are elbowed, it buries itself completely in the flesh, resulting in a red swelling, with a pustule containing watery matter. If the mite be not removed the irritation lasts several days, the pustule refilling as often as it is broken. It is seldom noticed, being so small, and the uninitiated are often alarmed at the symptoms. Saleratus water or salt water will allay the irritation, but sulphur ointment effects a speedy cure.

Another red mite, belonging to a different genus, makes itself useful by preying on insects. One species, *Trombidium parasiticum* (Fig. 2), is found on the house-fly, which in some seasons is so infested that hardly one can be caught that is not loaded with mites clinging to the base of its wings. This or closely related species are found on spiders and mosquitoes, which they help to kill off. One species Dr. Packard found doing good work in eating the plant-lice off his rose-bushes; another attack the Rocky Mountain locust, the grasshopper scourge of the West; they attach themselves to the body under the wings, and suck it to a dry shell; while still another species, *T. sericeum* (Fig. 3), creep in great numbers into the holes where the locusts' eggs are and eat them.

The *Gamasidæ* are for the most part parasitic on insects, as the beetle, humblebee, wasp, etc., though the kinds here noticed are found on warm-blooded animals. They are sometimes called ticks, and are closely allied to that family. The mite shown in Fig. 4, *Dermanyssus avium*, infests domestic poultry, canaries, and other cage-birds, living in henneries and aviaries, and ready, like many of its congeners, to migrate to persons the first opportunity. They are small, white, and quite agile. A case is related in which these mites settled upon the skin of a woman otherwise healthy. She was constantly infested with little, louse-like animals, which were supposed to be bred on her body, as the greatest cleanliness failed to extirpate them. It was at last found that she went several times a day into a cellar over which the hen-roost was located. As often as she passed the fowls flew up to their roosts, and by this means sprinkled the woman with mites. The removal of the hen-roosts cured her. Fowls, when allowed to roost in stables, sometimes convey these mites to horses, torturing the poor brutes nearly to death, the owner being unable to decide what malady afflicts them. The mite is easiest ob-

served in the cages of singing birds. That they feed upon the bird is shown by their digestive organs being full of blood. They harbor in the recesses of the cage and the perches, particularly those made of hollow cane, and sally out at night to settle on the sleeping bird.

FIG. 4.—DERMANYSSUS AVIUM.

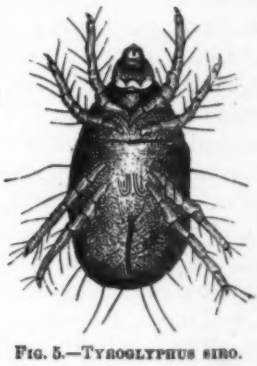


FIG. 5.—TYROGLYPHUS SIRO.

Scrupulous cleanliness should be exercised to prevent and remove them. Several members of this family are parasitic on cats. One species is found on dead bodies. A case has been cited where one was observed on the brain of a dead soldier, which had just been opened. Although the observer thought that the mite had been domiciled there, and upon the strength of this case the statement is often made that they are sometimes found in the brain, it is probable that the mite was in some way conveyed to the brain after it had been opened.

The *Ixodes*, or true ticks, are known by the tough, leathery skin of their abdomen and legs, their fastening on warm-blooded animals, and sucking their blood, for which they have a special mouth-piece. The body of the female is capable of great distention as she gorges. Their habit is herbivorous at first, for it is from the herbage that they find their way to the creatures on which they fix. They mount to the summit of blades of grass, or tips of leaves, and, holding on by their forelegs only, stretch the other three pairs out so as to fasten on any animal that comes in their way. Once settled, they plunge their proboscides into the skin, and suck the blood until their flat bodies become of a globular form, varying from the size of a number-eight shot to that of a bean. The process of filling is slow, sometimes taking days. Little irritation is felt, but the proboscis is liable to be broken off and remain in the wound, when serious sores are formed. Care should be used in taking them off; a little tobacco-juice will generally make them let go. Dogs after running in the woods often bring home quantities of them, and they may come to be permanently located in the dog-kennels. The common cattle-tick of the West is very annoying to horned cattle. Various troublesome species abound in the

tropics, and these exotic tick-mites are not infrequently imported on plants, and in the moss and sphagnum which horticulturists use in packing their wares. The habits of this group are somewhat anomalous; at first they are unquestionably herbivorous, but they greedily avail themselves of every opportunity of sucking blood instead of sap. No special adaptation is needed for this, as the sucking-apparatus is suited to both the liquid blood and the juice of plants, and is similar to that of all suctorial insects, which some use on plants, some on animals, and some on both. Of the immense swarms of ticks, gnats, mosquitoes, bed-bugs, etc., the majority probably never taste animal food at all, although so greedy for it when it comes in their way.

The sub-family, *Acaridæ*, is divided into two sections: the cheese-mites and their allies, and the itch-mites with their relatives. The first section produces a number of vegetable-feeders; several species are found on the scales of some species of *Liliaceæ*, principally hyacinths, and on dahlia-roots, cyclamen, potatoes, mushrooms, etc.; these occasionally cause itching and irritation to those handling them. The smallest species of this group, *Tyroglyphus entomophagus*, is a pest too well known to entomologists, upon whose collections it preys. It lives chiefly in the inside of the insects which it attacks, gnawing the soft parts of their bodies, and destroying the ligaments which hold the articulations together, allowing them to fall apart.

Edible mushrooms, especially the cultivated sorts, are often attacked by a moist black rot, which, until lately, has been regarded as spontaneous. But it has been shown that it is caused, or at least aided, by a species of mite remarkable for fecundity. The activity of their agency in this decomposition is shown by the fact that mushrooms on which they have been placed become, in forty-eight hours, a black, putrescent mass, on which myriads of these creatures swarm; while other mushrooms subjected to the same conditions, except the inoculation, dry up, and take from eight days to a fortnight to decompose. The presence of mites in great numbers on some of the common articles of food is well known. These are chiefly the cheese-mites, which are characterized by a soft, smooth, fleshy, whitish body, with generally a single claw, surrounded by a vesicle or fine sucker, like a sieve. *T. siro* (Fig. 5) is the principal mite which commits ravages upon cheese, living upon all kinds when a little decayed, and especially the harder parts. They hibernate during winter, crowded in heaps in chinks and hollows in the cheese; but with warm weather their activity begins, and they gnaw away, reducing the cheese to powder. This powder is composed of excrement in little grayish balls, eggs, egg-shells, larvæ, cast skins, perfect mites, fragments of cheese, and numerous spores of microscopic fungi. A mite-tainted cheese is not objected to by epicures, and the creatures are sometimes introduced to give a premature ripeness; but, if left to work unchecked, they will spoil a cheese very soon.

Linnæus described a mite under the name of *Acarus dysenteriæ*, to the presence of which was ascribed a persistent dysentery that attacked one of his pupils, and some later authors have thought it may have been the cheese-mite, or some of its allies; but this is improbable. Cheese-mites are constantly eaten in great quantities, and, so far as can be judged, with perfect impunity; if they did cause mischief, numerous cases would have been noted and reported, whereas there are none recorded. A mite is also found in flour, sometimes in abundance; Hassell, however, says it is never present unless the flour is damaged. Assuming it to be a separate and distinctive species, it was described by Linnæus and named *A. farinæ*, and has been so figured and spoken of by naturalists down to the present time. But Mr. Andrew Murray, whose excellent hand-book on "Economic Entomology" is freely used in this article, asserts that the flour-mite and the milk-mite are not distinct species, but are identical with the cheese-mite. Not, he explains, that every species found on cheese or flour is this species—for both are doubtless infested with others—but that the old authors have made two or three species out of one. The cheese-mite has been met with in very old linseed-meal, and has been found on wounds that had been dressed with poultices made of linseed-meal. Of course, its presence under such circumstances must be mischievous. Another species which lives on cheese, *T. longior* (Fig. 6), is distinguished from the above by its more rapid movements, larger size, and longer and rounder body. Its habits and diet are much the same, though they are not found together.

It is the species most commonly met with in stores of cantharides, which are very subject to attacks of mites. This species, Mr. Murray says, gave rise about 1837 to a good deal of talk among scientific people, as having been supposed to be produced by electricity. There was at that time in the semi-scientific world a vague idea in favor of electricity being the source of many of the phenomena of life. The limits and extent of its power were, of course, even less known than at present, and all sorts of wild experiments were tried. One gentleman set up lines of electrical wires over portions of his estate, with a view of ascertaining whether the plants would not thrive better under what he supposed would be an increased flow of electricity. Others made similar experiments in different directions. One gentleman, Mr. Cross, tried to produce organic beings by the aid of electrical apparatus. His process was to operate on volcanic stone kept moist by a solution of silicate of potash and muriatic acid, constantly subjected to electricity. After carrying this on for some time, he was rewarded by finding some of these mites wandering about his apparatus, and arrived at the conclusion that they had been produced by his electrical batteries. The species, therefore, enjoyed an ephemeral fame as a human creation. It was sent to M. Turpin, in Paris; having but a single dead specimen to work from, he believed it to be a new species,

but by no means endorsed the idea that it was created by Mr. Cross or his electrical apparatus. It was not only a highly organized animal, and nearly allied to a well-known species, but it proved to be a female, containing eggs, which, as he dryly remarked, seemed an unnecessary complication in a new creation.

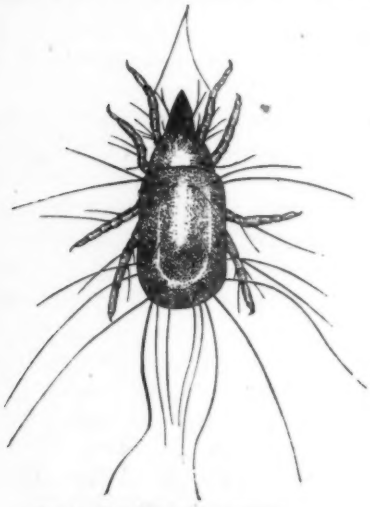


FIG. 6.—TYROGLYPHUS LONGIOR.

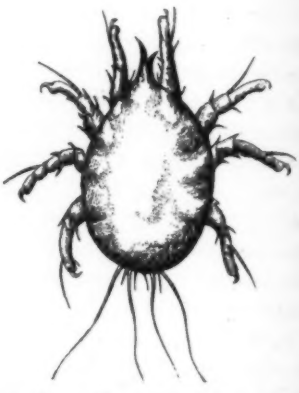


FIG. 7.—SUGAR-MITE (*Tyroglyphus sacchari*).

The sugar-mite, *T. sacchari* (Fig. 7), is most commonly found in brown sugars. It is large enough to be seen with the naked eye, and sometimes appears as white specks in the sugar. It may be detected by dissolving two or three spoonfuls of sugar in warm water, and allowing the solution to stand for an hour or so; at the end of the time the acari will be found floating on the surface, adhering to the sides of the glass, and lying mixed with the grit and dirt that always accumulate at the bottom. In ten grains of sugar as many as 500 mites have been found, which is at the rate of 350,000 to the pound. Those who are engaged in handling raw sugars are subject to an eruption known as "grocers' itch," which is doubtless to be traced to the presence of these mites. They are almost invariably present in unrefined sugars, and may be seen in all stages of growth and in every condition, alive and dead, entire or broken in fragments. Refined sugars are free from them; this is in part due, perhaps, to the crystals being so hard as to resist their jaws, but principally to the absence of albumen, for without nitrogenous matter they can not live. The sugar-mite is also found on the surface of jellies and preserves that have begun to dry, and on the sugar of dried fruit, such as figs, prunes, cherries, etc. They sometimes accumulate in the mouth of wooden taps used for wine and beer. In dismissing this part of the subject, which refers to mites infesting food, it seems proper to say that the

common assertion that living forms are a matter of course in everything that we eat and drink is without warrant. While it is true that water and one or two other articles usually contain microscopic organisms, the idea commonly conveyed by such statements is erroneous. These degraded and disgusting forms are not proper food-stuff; nor is their consumption unavoidable. Pure articles, in an undamaged condition, do not contain them; and their presence in numbers in any article of food is proof that it is unfit for human use, and should be rejected.

There are several species, placed by later authors in the same genus with the sugar-mites, whose normal habitat seems to be on food, insect collections, and in the dust and mold of cellars and damp places, but which, when transferred to animal bodies, become parasitic, causing curious and painful disorders. Signor Moriggia figures a singular horny excrescence which grew from the hand of a lady. It was nearly eight inches long, tapering upward from a wide base, and curved toward the wrist. Its cavities were swarming with a species of acari. Another species was found by Hering in the hind feet of a horse, that, although young and in other respects sound, had to be killed. The hoofs were quite disorganized, the frog and sole consisting of a soft, fibrous mass, secreting an offensive liquid; in the end, the sore spread to the flexors and muscles of the fetlock. A negro inmate of the Seaman's Hospital, London, suffered from large and peculiar sores on the soles of the feet. Examination by Prof. Busk revealed the presence of a mite, which was doubtless the cause of the trouble. The negro attributed the disease to wearing a pair of shoes which were lent for a few days to another negro who was similarly afflicted. The latter came from Sierra Leone, and inquiry proved the existence of a pustular disease, native to that place, called *craw-craw*, a species of itch very troublesome to cure.

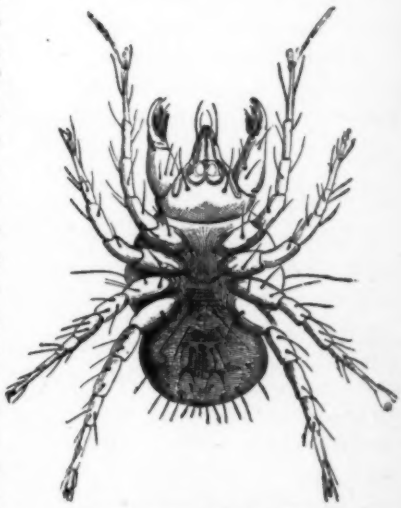


FIG. 8.—CHEYLETUS ERUDITUS.

The genus *Cheyletus* is a very remarkable type. It is unquestionably carnivorous, its palpi being adapted for holding its prey. They are not gregarious in their habits; but, like all animals that live by rapine, are solitary. When placed in company with the cheese-mites, they seize them between their palpi, and, plunging their beaks into the body, suck up the juices. The *C. eruditus* (Fig. 8) appears also

to have poisoning powers similar to those of the spider. They seize their victim by the leg or any other part, who, after the lapse of fifteen or twenty seconds, becomes paralyzed, makes no resistance to the devourer, and remains passive till nothing is left but an empty and shrunken shell. The poison has no effect on its own species: they frequently feed on each other; but in that case the prey struggles so long as any fluid seems to be left in the body. It is found in the dust of hay, old grain, meal, and flour, on collections, and also on books, from which latter it derives its name; but, in the opinion of Mr. Murray, it is so found because other gregarious acari congregate there, and "we should no more think that it was there for the purpose of feeding on those stuffs in which it is found than we should admit that a cat eats hay because it is found in a rat-infested hay-stack."

The occurrence of the phenomena of parthenogenesis among acarids has been shown by this species. Mr. Beck succeeded in rearing three successive generations from a female without any intervention of the male. The female is very careful of her eggs: she lays them in a heap, and rests brooding over them, guarding them from attack.

The itch-mites, *Sarcoptes*, infest the larger mammals. The commonest species, *S. scabiei*, which preys on man, when seen with the naked eye, looks like a white, shining globe, or a little bladder of

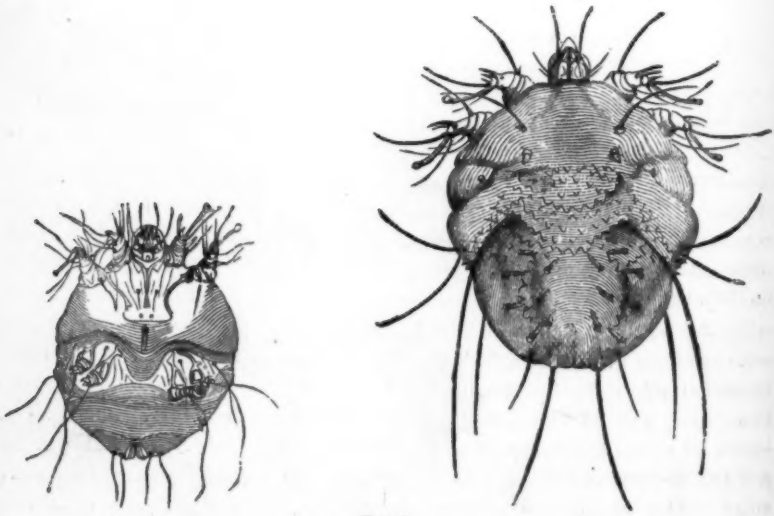


FIG. 9.

SARCOPTES SCABIEI—Male. SARCOPTES SCABIEI—Female.

water, $\frac{1}{75}$ of an inch long and $\frac{1}{100}$ of an inch wide. The microscope brings to view (Fig. 9) a round, flat body, with a head not unlike that of a tortoise, provided with powerful jaws and nippers, the structure of which is well adapted to mining the skin. The body is set with

strong, short legs, terminating in long spines (Figs. 10 and 11). The feet and nippers are provided with suckers. They have no eyes; but, with their habits of life, the absence of these organs causes them no inconvenience. The impregnated female digs a long burrow in the skin, depositing her eggs as she moves. If one of the early vesicles of the itch is examined, a small spot may be seen upon some point of the surface; this is the opening made by the mite to begin its tunnel; leading from this, a white, fluted line may be traced, which is the cuniculus or burrow of the acarus, and the fluted or dotted appearance is due to the eggs, the white dots showing where they lie (Fig. 12).

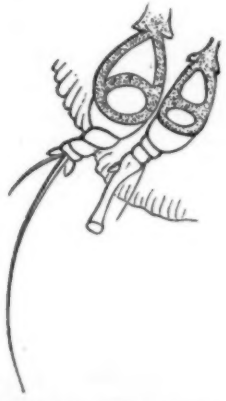


FIG. 10.—Two of the Posterior Legs of the Male *Sarcoptes scabiei.*

FIG. 11.—Two of the Posterior Legs of the Female *Sarcoptes scabiei.*

The burrow varies in length, sometimes reaching half an inch, and at its end, under a slight elevation, lies the mite. The cut shows a burrow with a number of empty shells scattered along the line, with one unhatched egg close to the mite. Close inspection will reveal a small, dark point at the end of the burrow; and, if the skin be raised there with the point of a needle, the creature can be brought to view and easily extracted. The itch-mite is never found in a vesicle or pustule; indeed, there is no connection between the later vesicles and the burrow; they are evidently caused by the proximity of the mite. The larvæ and young females hide themselves in broken surfaces, or burrow a short distance into the skin; the male retreats under any protecting edge. The advance of civilization, with its increased use of soap and water, has done much toward exterminating this pest; the "Jackson itch" and "seven-year itch" are much less heard of than formerly; but wherever, as in army life, men are crowded together, and personal cleanliness is neglected, it reappears in a most flourishing condition. The disease must, of course, be conveyed by transference of the mite from those who are infected to those who are sound;

but the mode of transmission has been a puzzle, it being observed that among doctors and hospital attendants infection was comparatively rare. This is explained by the nocturnal habits of the mite—or, if not truly nocturnal, its activity is promoted by a certain degree of warmth, so that it lies dormant during the day, when the body is cool, to sally forth in search of new fields when stimulated by the genial warmth of the bed. This explains also why the itching is most violent at night. The mode of getting rid of the parasite is obvious; we have only to kill it; and, fortunately, the means is cheap, easily applied, and perfectly effectual: sulphur, which seems to be a deadly bane to all insects, in whatever shape it can be given. It is only necessary to expose the insect to its influence, and it surely dies.

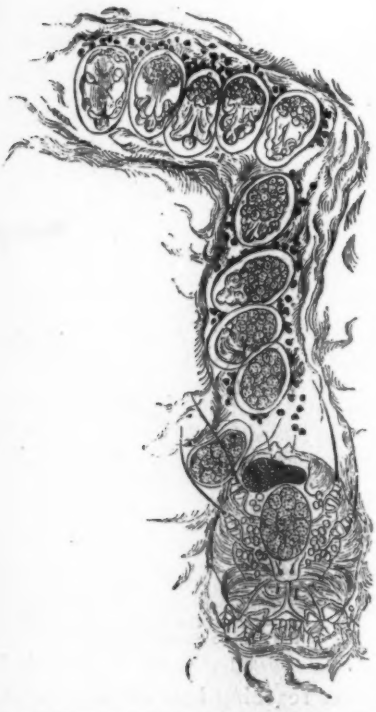


FIG. 12.—BURROW OF ITCH-INSECT (*Sarcoptes scabiei*).—Female depositing eggs. The eggs lying next to the insect consist partly of an homogeneous, partly of a granular mass; in those distant from the insect embryos are already developed, and at the entrance of the furrow a moving acarus is seen.

One curious point regarding these mites is, how they produce the physiological effects that characterize their attacks. Their incessant gnawings and nibblings are undoubtedly sufficient to cause great irritation; but there are the different symptoms induced by different species—scurfy, inflamed surfaces, coarse, leprous crusts, deep ulcers, etc.; can these be accounted for by degrees of mechanical irritation, or are they possibly the result of some special poisonous virus? The itch-mite is found on the lion, dog, sheep, ox, horse, pig, and other animals, as well as on man. There are several other species, as the *S. scabiei-crustosæ*, or Norwegian itch-mite, most common in Norway, but not wholly confined to that country. The mite is much like the common species, but smaller and darker. The malady is similar to that of the common itch, though all the symptoms are intensified, the tubercles and crusts growing enormously. It yields, however, to cleanliness and the sulphur treatment. *S. cati* infests the domestic cat, the points of attack being the base of the nose, the lips, the ears, and the eyes, but spreading in all directions; the animal is reduced to a pitiable condition, being literally devoured alive by the parasites, if not de-

stroyed before this extreme stage is reached. This complaint is chiefly seen in great towns. *S. mutans* (Fig. 13) is parasitic on birds and domestic fowls, appearing on the feet, on the comb, and about the beak, gradually spreading until the whole body is involved. Other species have been found on other domesticated and wild animals, while a large genus —the louse-mites, *Myobia*—infest the smaller mammals, as rats, mice, cats, etc.

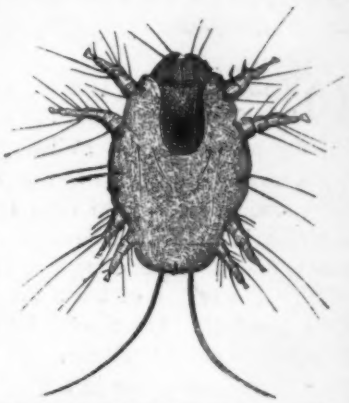


FIG. 13.—SARCOPTES MUTANS—Male, underside.

The *Demodex folliculorum* is a minute animal, $\frac{1}{100}$ to $\frac{1}{50}$ of an inch in length, that lives in sebaceous sacs and hair follicles of the human skin, particularly about the nose (Fig. 14). Its habits are in some respects similar to those of the itch-mites. It is not a normal inhabitant of the follicles or glands, but appears to enter them from without. If pressure be made upon any one of the sebaceous follicles that are enlarged and whitish, with a terminal black spot, matter will be forced out, consisting mostly of accumulated sebaceous secretions, in which the parasite, if present, will be imbedded together with its eggs and young. The secretion may be softened with oil, so that these may be separated and the animal removed with a pointed brush. They do not seem to be present in all persons, occurring in two or three cases out of ten, and seeming to prefer thick, greasy skins. They are entirely harmless, and their presence is no indication of disease. The family roll of the acari closes with the gall-mites, *Phytoptidæ*, a curious and little-known group that causes the abnormal growths known as galls on the leaves and other parts of various plants. Any extended reference to them here would be out of place. The whole of the order *Acarina* afford interesting objects of investigation, being in close relations to man, and yet very imperfectly known. Some of their forms are always attainable, and an examination of them under the lower powers of the microscope is one of the most amusing objects of study that can be presented to the young.

FIG. 14.—DEMODEX FOLLICULORUM.

TYPHOID-FEVER POISON.

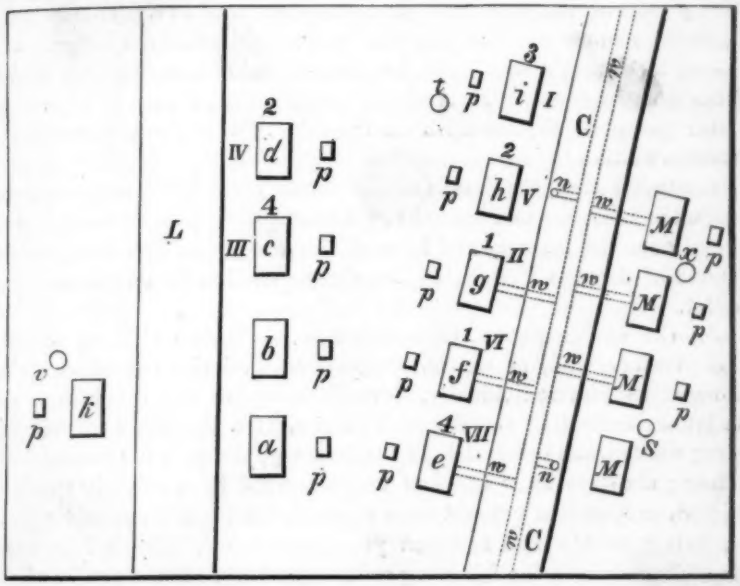
By ELY VAN DE WARKER, M. D.

ON the 4th of September, 1876, Otto Schmidt, an industrious and thrifty German, reached his home after an absence of a week at the Centennial Fair at Philadelphia. How and where he lodged, and what he ate, during the five days he was at that city, we have no means of knowing, for, by the time he had exhausted the marvels of the exhibition in his voluble German tongue, he had lost all idea of unity of place, and was wandering mentally amid the busy wonders of Machinery Hall. The reason of his mental migration was a very simple matter. Otto was sick. On the 8th he complained of headache, bodily prostration, and mental lassitude. The next day the distress in his head was very severe, with pain in the back and limbs, and chilly sensations. The day following there was fever with loss of appetite, and toward evening he vomited the only food he had taken that day. On the 11th he thought he was better; but on the 12th the fever, both morning and evening, was marked. The next day he was seen by a physician, and the disease was recognized as typhoid fever. And here, having given in brief the history of the misfortune that had befallen Otto, let me describe the little segment of the world that held all that was dear to him in the way of family and friends. In order to appreciate what follows, every point and detail in this miniature survey must be understood and remembered.

No reader of history can follow the marching and countermarching of hostile armies without his map; and here also, upon a space of ground one hundred and sixty-five by five hundred feet, we have the invasion of an active, subtile, invisible foe after due declaration of war. To the reader and myself this narrow limit of ground is historical. Here was fought one of the most destructive battles ever recorded in the annals of human misery—a campaign between this imponderable invader and the vitality of a score of human beings. This conflict was waged unheeded by the dense population around, which seemed bent more on enjoying itself during the pleasant harvest weather than witnessing an unequal fight.

The —— ward of the city of Syracuse is built in its northern extension upon a series of parallel hills and valleys. These hills, with steep eastern and western flanks, govern in a measure the direction of the streets. There are three of these crests, with corresponding valleys. The locality indicated in the cut is situated in the central depression. Directly to the west it is commanded by the second ridge, the sides partly built up, partly terraced, and the summit is crowned by Lookout Park, a pleasure-ground of doubtful promise, its scanty verdure being checked in its growth by the stony soil and the violent winds that

sweep the summit. The slope of the hill east of the infected locality is of much lighter grade than that of the western ridges, and is thickly built up with small frame houses, except that about one hundred yards south of the group of houses, shown in the cut, the slope of the ridge is covered to the extent of twelve acres by the city cemetery, filled by



EXPLANATION.—*a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, *i*, *k*, houses; *L* and *C*, streets; *w*, *w*, sewer and house drains; *p*, privies; *s*, *t*, *u*, *v*, wells. The Arabic numerals attached to the houses indicate the number of cases of fever in each. The Roman numerals indicate the order in which each house was invaded by the fever. *n*, sewer opening in the street gutter. *M*, uninfected houses.

eight thousand interments, and still used as a burying-ground. This second valley differs from the other in being gradually narrowed in its northern extension, so that the streets *L* and *C* gradually approach and enter each other at an acute angle about one hundred yards north of the scene of the infection. The little group of houses shown in the map are isolated by vacant lots to the north and south. The houses were one story frame, gable ends faced to the street, neat and comfortable, and were, with one or two exceptions, little freeholds, occupied by their owners, tidy and industrious Germans, except that the houses *f* and *g* were occupied, one by English and the other by Irish-American families. The houses were built two upon each city lot of sixty-six feet front by one hundred and twenty feet deep, and separated from each other by light picket and board fences. Several of the owners turned their ground to account by growing vegetables. At an average distance of twenty feet to the rear of each house were the usual privy-vaults, all of them too shallow and badly cared for. The water-supply

was very defective, as this portion of the city was not supplied by the city water company. Well-water was therefore used for drinking and cooking purposes; while for washing, rain-water was caught either in cisterns or hogsheads. The water for culinary use was obtained from three sources. The houses *g*, *h*, *i*, *c*, and *d* obtained water from the well *t*; the houses *a*, *b*, *k*, from the well *v*; while the people in houses *e* and *f* crossed the road and patronized the well at *s*. This was the customary manner of supplying their wants. The source of supply at *s* was cut off during a short time, but, as this incident gave a clew to one of the most interesting facts of the investigation, I shall consign it to a later period of the narrative for the sake of what the critics call the dramatic unities.

Assuming that the details of the locality are sufficiently clear to enable the reader to understand the different steps of the search, I shall give at once the marches and halts of the fever in its invasion, and the amount of damage it inflicted on the population of our intra-mural hamlet.

As the reader knows, Otto Schmidt was taken sick on the 8th of September, and, as we have given him all the importance that belonged to him individually, I shall designate the other cases by the letters indicating the different houses. On the 4th of October a young woman was taken sick in the house *g*, the only case in a family of four; six days later the first case occurred in *c*, a family specially afflicted, as here four persons were stricken, leaving but two of the family, girls aged ten and fourteen years, untouched. These four cases were taken sick in the following order—the 10th, 14th, and 23d of the month; on the 20th and 26th we have two other cases in the house *d*, and two cases, at intervals of eight and twelve days later, in the house *h*, near neighbors of the Schmidts. In the house *f*, at nearly the same time, a case of the fever occurred. The last family attacked resided at *e*, and here the disease seemed to linger with special animosity, the first person being attacked on October 28th, and the last on November 8th. At this place and date the disease expended its force. In Otto Schmidt's family there were two additional cases taken on the 16th and 20th of October. In the total there were seventeen cases; five were very light, what some authors have called "walking cases." In the remaining twelve cases there were three deaths, one from an intercurrent pneumonia, or lung inflammation, one from intestinal hæmorrhage, and the other, without any special complication, gave way before the onslaught of the poison.

In an investigation of the kind before us we must have a knowledge of all the circumstances, and therefore, even at the risk of weaving such a complicated array of events around this drama of disease that it may need the skill of a professional novelist to disentangle us from the meshes, we must glance for a moment at the condition of the weather during at least the early part of the outbreak. From about the begin-

ning of September a period of continuous dry weather characterized the month ; there may have been one or two slight showers, but not sufficient to change the general condition of dryness of earth and air. During this drought the well at *s* gave out, and continued dry until replenished by the late fall rains. The wind, blowing generally from the west and northwest, served to keep the temperature down, but not to lessen the unpleasant dryness of the air. This was changed on the 20th by a thunderstorm from the west, repeating itself from the east after a brief interval with nearly equal force. So violent, however, were the wind and rainfall, that the water was carried off by surface-wash, filling up sewers and cellars and tearing out gutters, but doing harm rather than good to vegetation. The drought was ended, however, as this was followed by a series of easy showers. During October there was much wet and cold weather, the meteorological complexion of the late fall being decidedly unpleasant.

Now that we are ready to begin the story of our search, what shall we look for ? We have here a circumscribed outbreak of typhoid fever, a disease admittedly of an infectious nature, but proceeding from what kind of infection ? Shall we content ourselves with finding a heap of decomposing vegetable or animal matter, or rest satisfied by discovering an escape of sewer-gas from some untrapped drain into one or more of the houses ? Since we—the reader and I—are scientific and reasonable people, we can not adopt the theory that the disease is contagious—an opinion held by some able but rather easy-going physicians—and having once effected a lodgment in the community is capable of infecting all who have systems favorable to the development of the poison. The disease is not contagious in the sense that small-pox and scarlet fever are. We have proof in the outbreak we are studying that it is not of such a nature ; but we have further proof. Liebermeister says that he had never seen in the hospitals of Greifswald, Berlin, and Tübingen a single hospital patient, physician, or nurse attacked with typhoid fever, although such cases were placed in the general wards. Dr. Murchison states that during a period of fourteen years 2,506 patients were treated for typhoid fever in the London Fever Hospital, and yet during this long period, and notwithstanding this vast number of patients, only eight cases originated in the hospital. It is needless to cite further evidence, and, since we can not save ourselves hard work by the slipshod theory of contagion, we must explore further.

Sewer-gas and decomposing matter we can not dismiss in this summary way; for here we are entering upon the special preserves of boards of health, sanitary engineers, and all others who advocate the gospel of cleanliness. In admitting or excluding sewer-gas as an element in the spread of the disease, we need a more complete array of conditions than is furnished by our group of houses; but such evidence as is wanting we must procure from other sources. A glance at the cut will show that but three houses in the fever-group were connected

with the street sewer. Two of these, *e* and *f*, were drained from the cellars by cement pipes four inches in diameter, the slops from the houses being thrown upon the ground. The house *g* was drained both from the cellar and by a branch pipe, untrapped, leading to an open sink by the side of the kitchen door. Two of these houses were the sixth and seventh in the order of invasion by the fever. There can be but little doubt in the event of heavy rains and sudden change of temperature that sewer-gas escaped into these houses. I never was able to detect such an escape of gas, but I concede the fact from what I know of city sewerage. If, however, we accept the explanation of the propagation of the fever after its first introduction in the person of Otto Schmidt by means of the sewer-gas, we leave unexplained a very important circumstance. In the group of infected houses but three out of seven were connected with the street sewer, so that in this particular outbreak sewer emanations are not competent to explain the extension of the disease. We may throw yet further doubt on the sewer theory by the fact that in the cottages *M*, containing numerous inmates, we have a group of uninfected houses, three of which were directly connected with the same sewer. If we were seeking for the cause of typhoid fever in defective drainage we should select these uninfected houses, as they not only had untrapped sewer pipes in the kitchens, which were practically the living-rooms of the families, but they were built at the foot of the slope and received the surface-wash of the rich alluvium of the hill to the west. Notwithstanding these unhygienic conditions and the actual existence of seventeen cases of the disease on the other side of the street, we find here complete immunity. When we have considered what this fever-poison really is, we shall be in a position to understand the origin of the disease from this source.

In seeking for a cause of the rapid spread of the fever in decomposing matter we are still at fault. The houses were all new, none of them having been built longer than five years. About none of them was found anything like an accumulation of refuse matter. They were careless about disposing of the water from wash-tubs and what is familiar to every housewife under the name of "dish-water"; but these were disposed of with more care than usual, as owing to the dry weather they were scattered over the gardens. In the condition of the vaults a critical inspector could find fault, yet they were in as good condition as these very defective household appendages usually are. Admitting a focus of infection at the house of Otto Schmidt, it is difficult to conceive of such a wide diffusion of the virus as to suddenly establish a focus at each of these seven points. This, however, is one of the least of the difficulties met with in tracing the disease to the decomposition of animal or vegetable matter. In typhoid fever we have a specific disease; that is, it exhibits a well-defined and uniform manner of beginning, a culmination and decline equally well marked, and, taken together, a group of symptoms and anatomical lesions that define

typhoid fever and no other disease the world over, and under all conditions. The question comes up—and the pythogenic (putrefaction) theory must be competent to afford an answer, or it will have to be abandoned—In what way can an infection, which must be composed of many heterogeneous and opposing elements, and produced under widely different conditions, be capable of producing in different individuals, each affording a nidus for the development of the poison under an almost infinite diversity of circumstances, one uniform train of symptoms, one specific, well-marked disease? The answer to this is, that it depends on the kind of matter decomposed. To human excrement, above all others, has been given this fatal power. To this recently sewer-gas, which we have already excluded from the chain of possible causes, has been added. But typhoid fever has never been produced by experiments with decomposing substances, nor by products of decomposition accidentally introduced into the human body; and, further, it is opposed to our daily experience. There are vast numbers of houses in which the affluvia of vaults may be detected in all the rooms, others in which the inmates are constantly inhaling sewer-gas, and the fixed as well as the transient inhabitants escape the disease. It is safe to say that cities with defective sewerage are the rule; that some cities and villages are more filthy than others; that in some quarters of a city the decomposition of organic and excrementitious matter is constantly going on; and yet we find typhoid fever prevailing independently of all these theoretical sources of infection: we find it localized in one section, even in one block of buildings, while others presenting equally favorable conditions have not produced a case within the memory of man.

The theory has been seriously damaged by its friends. Not only typhoid fever, but a series of specific diseases, of the origin of which we are in a measure ignorant, have been referred to the decomposition of organic substances. Dysentery, yellow fever, cholera, typhus fever, and the plague have been assigned to this cause. Liebermeister says that this "very circumstance shows that to explain the origin of typhoid fever by a general and indefinite assumption of a decomposition of organic substances is not satisfactory. It is not every kind of decomposition that can produce typhoid fever; it must be some specific form of decomposition which elaborates as a specific product the poison of that disease."

In the epidemic which we are studying we have excluded the theory of contagiousness. We saw in the conditions reasons for excluding the sewers as a means of extension; and I trust, from what I have said of the nature of the disease-germs, the reader is willing to admit that I have some reason for the belief that we can not find an origin *de novo* in the house-vaults.

The atmosphere in this epidemic is not a probable means of the extension of the germs. We are not able to say positively that the

air will not convey the poison, although it is safe to say that the area of its diffusion by this means is very limited. Instances in which sewer-gas has been known to be the means of conveyance are numerous, but the cases were confined to one or more rooms directly communicating with the sewer, or to a limited part of a building. But the typhoid-fever poison is not a gas. In such a shape we can not conceive that it will lie dormant in suitable soil, that it will undergo phases which so closely resemble germination and growth, or that it can be transmitted to long distances when given a proper vehicle. We have here a right to make a scientific use of the imagination. We can not imagine it a gas, but we can form an idea of it as an atom, a germ capable of preservation, growth, and infinite multiplication. It has never been seen; it may be that it will never be seen. In the physical sciences we project ourselves ideally into the midst of many things unseen, yet with perfect theoretical conviction in the reality of their existence.

In the group of cottages to which the epidemic was confined we observe a certain order in the arrangement of the infected buildings. A close examination of the plan is nearly sufficient to convince one that this order is not due to chance. Nor can we explain it by a conveyance of the disease-germs in the clothing of the inmates. We have in the exempt houses a sufficient warrant against this supposition. The inhabitants of the exempt houses, as the outbreak of the fever extended, assumed generously nearly all the care of the afflicted families. They were daily and nightly in attendance upon the sick, and in many cases assisted in washing the linen. If this were true of the inhabitants of the houses that were exempt from the scourge, we are nearly safe in excluding personal conveyance of the germs by those who inhabited houses showing a sequence of infection.

During my attendance upon several of the cases I repeatedly looked over the ground and studied the habits of the different families, but, I am ready to confess, without discovering the clew to the extension of the disease from the single focus in the person of Schmidt; that is, to discover a clew that would meet the demands of the scientific germ theory. However, I became convinced that if I were able to find some domestic arrangement or prevailing condition that linked the infected families together under a common liability to the disease, I should become master of the position. Such a connecting link I found in the water-supply of the different families. I did not make the discovery accidentally, but only as a result of a systematic investigation. In all researches of this kind I believe that it is rare that the truth is stumbled upon in the course of a careless search. If we do not exclude cause after cause in the course of the search, we are almost sure in the end to halt midway between two probable conclusions without being sure of either.

It is necessary to recall the fact that five houses, *g*, *h*, *i*, *c*, and *d*, were supplied with water from the well *t*—an open curb-well, loosely

stoned up, belonging to Otto Schmidt. All of these houses were invaded by the fever. But it is necessary to account for the contamination of the water. Here comes in the fact that gave me the key to the mystery. On the 20th of September, as I have already stated, there occurred a severe thunderstorm. It was so violent that a large amount of the rainfall was carried off as surface-wash. It did considerable damage to the gardens, and, among other things, filled up to overflowing the vault attached to Otto Schmidt's house. Here was deposited all the excrementitious matter from this severe case of the disease. It was easy to observe, from the appearance of a board walk, of the grass and ground, that the overflow had scattered material of this kind in the neighborhood of the well. Whether it ran into the well, or filtered through the ground, it is difficult to say; either could have occurred, or possibly both.

Now observe what occurred. Fourteen days after the shower, the 4th of October, a case of the disease broke out in the house *g*. Twenty days after, the first case was taken down in the house *c*; thirty and thirty-six days later, two cases in *d*, and two cases at intervals of six and twelve days later in the house *h*. These were the families habitually dependent on the Schmidt well for their supply of water.

Admitting that I am right in tracing these cases to the contamination of the well *t*, by the shower of the 20th of September, we have left two families in which occurred five cases of the fever who did not make use of this well. On questioning the people in the houses *e* and *f*, I learned that they crossed the road and drew water from the well *s*. This missing link in my chain of cause and effect made a halt in my investigations. I had fancied that I was so near success that I took this disappointment as a sort of personal matter, and was half inclined to give the whole thing up. I made another effort; I crossed the road and investigated the well and its owner. There was no fault to be found with the well, which was a "drive well"; but the owner told me that it occasionally became dry, and was in that condition before and for a week or two after the great shower of September. Back I went to my fever-houses, and exploded this question like a bomb-shell in the midst of their inhabitants: "Where did you go for water when the Bogert well was dry?" "Oh, we went to Mr. Schmidt's well."

After this testimony let us trace the course of the invasion a step further. The fever appeared among the inmates of the house *f* on the 20th of October, and of the house *e* on the 28th. Had we no further evidence, here is sufficient to render probable that these cases were due to drinking the infected water from the Schmidt well. But we may strengthen it by negative evidence. The inhabitants of the uninfected group of houses on the opposite side of the street at *M* never used the suspected well. When the well *s* became dry they resorted to the well at *x*. There is one other group of uninfected houses, two of them belonging to the same block as the infected houses, and one situated on

the opposite side of the street *L*. These three houses contained fifteen inmates, all of them coming into frequent contact with the sick. These people were supplied with water from the well at *v*, and were throughout the epidemic exempt from the disease. Thus, to sum up, all the families that were supplied from the Schmidt well after the 20th of September had one or more members attacked by the fever; all the families that were furnished with water from other sources escaped. The conclusion is reasonable that in the drinking-water we have the means of the diffusion of the germs.

I have here shown a direct connection between the case of Otto Schmidt and that of every other infected person. This connection, traced through the drinking-water, was a discovery in our own epidemic; but it is by no means a discovery as a means of diffusion of the disease. This has long been known, and the literature of the subject is full of instruction.

It must be borne in mind, however, that not all water contaminated with organic impurities will cause typhoid fever. There must be among these impurities the specific poison of the disease. And this, as we have been able to prove, can be traced to former foci of typhoid fever. Other fluids may be contaminated in the same manner. An interesting report of an epidemic of the disease in England was given in the "Lancet" several years ago. A pasture had been manured by sewer products. Here were fed a large number of milch cows. In a neighboring town typhoid fever became rife. The victims, it was discovered, all used milk from this dairy; those who did not make use of it escaped. One can not help speculating about the way in which the disease-germs entered the milk. The more probable explanation is a mechanical one: that the germs adhered to the udders of the cows and were brushed off in the act of milking. It hardly seems reasonable that the germs passed through the organism of the cows and thus entered the milk. The theory of a *contagium vivum* is further fortified by the circumstance that, by boiling, the power to harm is taken away from the infected fluids.

It must be observed that between the entrance of the infecting elements into the system and the breaking out of the symptoms which define the disease, a varying period of time intervenes. This is called the period of incubation. It is a feature which distinguishes the entire group of infectious diseases. In determining the length of this period there are two elements of uncertainty: first, it is difficult to fix the exact time of infection; secondly, it is often impossible to define the exact beginning of the disease. In typhoid fever, especially, the onset is insidious and slow. Days and even weeks may be passed in a depressed, languid state of the health, before the disease is recognized as typhoid fever. Among business men these initial symptoms are often explained by taking cold, or by overwork. During the epidemic at Basle a few persons were attacked after a residence in the city of from seven to fourteen days; others after sixteen days. Haegler found in

three cases caused by drinking-water a period of incubation of twenty-one days. In our own epidemic one case occurred in fourteen days after drinking the water, the average time being about twenty days. The general experience at the present time points to an incubation of about three weeks; but it may vary from two to four weeks, or even longer.

It is an interesting question, but one to which I cannot give a satisfactory answer, Why do not all who are exposed to the infecting cause suffer from the disease? In some cases it may be due to idiosyncrasy. Some people resist powerfully the encroachment of all infectious diseases, while others seem fated to have an opportunity of testing in their own person every prevailing malady. Experience teaches, however, that typhoid fever, unlike many other diseases of the group, is favored in its development by unhygienic surroundings. Bad air, bad food, uncleanliness, and over-population of a house or quarter of a city, create a condition in the system favorable to a rapid and virulent development of the fever after an infection by the germs. We know, nevertheless, that no house nor person is exempt from the disease after receiving the exciting cause. Prince and peasant alike have to bow to the malignant potency of these infecting germs.

SKETCH OF ELISHA GRAY.

By GEORGE B. PRESCOTT.

ELISHA GRAY, the inventor of the Speaking Telephone, was born at Barnesville, Belmont County, Ohio, August 2, 1835. During his boyhood he was profoundly interested in all the phenomena of nature, and had an intense desire, whenever he saw any manifestation of physical force, to become acquainted with the secret of its operation. Among all the phenomena throughout the domain of physics, nothing took such hold upon his mind as that exhibited in the various effects produced by the action of electricity, and he read whatever he could find relating to this subject with the same eagerness and interest that most boys would read "Robinson Crusoe" or the "Arabian Nights."

While yet a boy he constructed a Morse register, all the parts of which were made of wood, with the exception of the magnet, armature, and embossing point in the end of the lever (which latter he made by filing a nail down to a point). He had the magnet bent into a U-form by a blacksmith, and then wound it with brass bell-wire, which was insulated with strips of cotton cloth wrapped around it by hand. For a battery he made use of a candy-jar, in which he placed coils of sheet copper and zinc, with a solution of blue vitriol. With these materials he succeeded in making a very good electro-magnet, which would sus-

tain nearly a pound weight, and which, when mounted as a part of the instrument, performed the work of actuating the armature with perfect success.

At quite an early age he was apprenticed to a blacksmith, and worked with him at that business about one year. He found, however, that this business was too laborious for him, and relinquished it to become an apprentice to a carpenter, joiner, and boat-builder, with whom he served a full apprenticeship, during which time he was employed in almost every department of woodwork. The prime motive which actuated him through all these years that he had worked at the bench was his thirst for knowledge. He felt sure that, with his trade as his capital, he could work his way through a course of study. In pursuance of this idea, after the time had expired for which he had apprenticed himself, he began a regular course of study, and, by working a portion of each day and during vacation at his trade, was enabled to pay his necessary expenses and keep up with his class. Here, as everywhere else, the capacity and ability to master everything relating to physical science was perhaps the most prominent characteristic exhibited during his collegiate course. While studying natural philosophy, it was his custom to make and carry with him into the class such apparatus as could be readily constructed and would serve to illustrate the lesson. His habit of actually constructing everything which he saw or read of, so far as his facilities would allow, was the best possible method of fixing the principles of its operation firmly in his mind.

Mr. Gray's career as a professional electrician and inventor dates from the year 1865. His first patent for electrical or telegraphic apparatus was granted October 1, 1867. Since that time he has made a considerable number of electrical inventions, many of which have been patented. Including cases now pending, the number amounts to about forty in this country and thirty in foreign countries. Thirty of the United States cases and twenty-five of the foreign relate to the telephone.

His attention was first called to the subject of telephonic transmission in the winter of 1867. In the course of his experiments during this winter and spring, he made use of a vibrating electrotome or reed in the primary circuit of an induction coil, and an electro-magnet having a polarized armature, in the secondary circuit. A Morse transmitting key was also inserted in the last-mentioned circuit. When the electrotome was in operation, and the Morse key in the secondary circuit was closed, he noticed a singing sound in the electro-magnet, and, by working the key as if transmitting a Morse message, the signals were audibly produced on the magnet by long and short sounds, representing the dots and dashes of the Morse alphabet. He saw in it a method for transmitting signals for telegraphic purposes, and also about the same time conceived the idea of arranging a key-board, having electrotomes tuned to the different tones in the scale. He did not, however, at this

time realize the full value of the invention, as his attention was mainly directed to the capacity of the apparatus for transmitting musical tones through an electric circuit.

In the winter of 1874 Mr. Gray observed a singular circumstance in connection with the reproduction of electrically transmitted vibrations through the medium of animal tissue. On going into the bath-room he found his nephew playing with a small induction coil—"taking shocks," as he expressed it, for the amusement of the smaller children. He had connected one end of the secondary coil to the zinc lining of the bath-tub, which was dry at that time. Holding the other end of the coil in his left hand he touched the lining of the tub with the right. In making contact, his hand would glide along the side for a short distance. At these times Mr. Gray noticed a sound proceeding from under his hand at the point of contact, which seemed to have the same pitch and quality as that of the vibrating electrotome, which was within hearing. Mr. Gray immediately took the electrode in his own hand, and, repeating the operation, to his astonishment found that, by rubbing hard and rapidly, he could make a much louder sound than the electrotome was making. He then changed the pitch of the vibration, increasing its rapidity, and found that the pitch of the sound under his hand was also changed, it still agreeing with that of the transmitted vibration. He then moistened his hand and continued the rubbing, but no sound was produced so long as his hand remained wet; but, as soon as the parts in contact became dry, the sound reappeared. So striking was the effect that, by hard rubbing with the dry hand, the noise could be distinctly heard throughout the house.

This experiment produced a profound impression upon his mind, and determined him at once to take the matter up in earnest and see what might be in it. He procured a violin, and, taking off the strings, substituted in their place a thin metal plate provided with a wire connection, so that he could attach it to one pole of the induction coil or battery, thus placing it in the same position, with reference to the body, that the bath-tub was in the original experiment. By rubbing the plate in the same manner as before described, the sound of the electrotome was reproduced, accompanied by the peculiar quality or timbre belonging to the violin. He noticed, however, that the characteristics of the initial vibrations were faithfully preserved, and all that was needed was to sift out such foreign vibrations as were excited in the receiver, owing to its peculiar construction; in which case there would remain the exact character of the transmitted vibrations.

In March, 1874, Mr. Gray undertook to secure letters patent for some of his conceptions, and with that purpose in view had models made, illustrating the idea of a series of transmitters, each tuned to a different pitch, showing a method of receiving musical or other sounds telegraphically, through the medium of animal tissue. In June, 1874, he filed another case, substituting for the animal-tissue receiver an

electro-magnet combined with a hollow box of tinned iron, having an opening in one side, while the other was held over the poles of the magnet at such a distance from it as would produce the best effect. With this apparatus he noticed that when he depressed two keys on his transmitter, if these were in the proper relation to each other, a composite tone would be received, thus demonstrating the general fact that, with a transmitter and receiver properly constructed and arranged in the circuit, composite tones of varying quality could be telegraphically transmitted and received.

When the fact dawned upon him, and had been confirmed by demonstration, that sounds of a composite character could be transmitted through a telegraphic circuit and reproduced at the receiving end, and the possibilities of the invention and the great results to which it must eventually lead passed through his mind, he at once foresaw so many possible applications of it that it became a serious question which line of investigation to first pursue. Among other conceptions of the probabilities of the invention was that, at an early day, not only musical compositions of a complicated character, but even articulate speech, would be transmitted through a single telegraph wire. He could also see that musical tones, differing in pitch, could be simultaneously transmitted through the wire and analyzed at the receiving end, so that a transmitter and a receiver correspondingly tuned would transmit and receive a tone corresponding to their own pitch, rejecting all others; while at the same time a number of other tones differing in pitch might be simultaneously transmitted and received through the same wire. This he successfully accomplished, sending as many as eight messages simultaneously. Another conception which occurred to him at this time was that of applying the invention to a printing telegraph, so that each type would be actuated by a tone of a particular pitch. Being well conversant with the facts, so far as they were then known in the science of electricity and magnetism, he was fully prepared to avail himself of what had already been done in that line. He was not, however, experimentally conversant to the same extent with the facts in the science of acoustics, but theoretically the subject was a familiar one to him. He devoted considerable time to familiarizing himself experimentally with that science, especially that branch which related to the qualities of composite tones; so that he was able to give the composition of the various vowel sounds, and determine in general the relation between the character of a sound as it seemed to the hearer and the physical fact as it existed in the form of motion, either in the air or any medium through which it was propagated.

The early part of 1874 he devoted principally to the construction of various devices for telegraphically transmitting musical tones. Among the receivers which he used was an electro-magnet with a circular diaphragm made of a thin sheet of tinned iron. It will be observed that this instrument embraces all the substantial features in the mechanical

construction of the speaking telephone of to-day. When used in connection with his articulating transmitter, which was developed at a later date, articulate words have been received upon it; and when a duplicate of the instrument is inserted in a closed circuit, which includes a galvanic battery, it becomes a speaking telephone capable of acting both as a transmitter and as a receiver. Mr. Gray did not know at that time, however, that he could use it as a transmitter, although he had fully demonstrated its ability to receive sounds of varying quality. At that date his conception of a transmitter for the transmission of articulate words was a mechanism which would employ such tones as were needed, and would enable one to manipulate them in whatever manner was requisite to produce the desired effect. In other words, he supposed it would be necessary to construct a mechanism similar to the vocal organs of the throat, which would mold electrical waves into the same form that the air is molded when a spoken word is uttered. This seemed too complicated a machine to be easily constructed; hence he determined to experiment particularly in the direction of the more perfect transmission of composite tones, so that each individual tone would have its individuality and place properly preserved in the clang of which it was a part; and to the analysis of the same at the receiving end, so that any particular tone would respond upon one instrument, and one only. This general result once attained, it was his purpose to make an application of it to multiple, printing, and autographic telegraphy. While engaged in these experiments he was continually on the alert for developments that might assist him to solve the interesting problem already before his mind, that of transmitting spoken words. Shortly after he constructed a transmitter, consisting of a revolving shaft, upon which were mounted two eccentric cams, having one or more projections. These actuated two small levers, causing them to vibrate upon their respective break-points, through which points a battery current passed. He employed, in connection with this transmitter, a receiver which was adapted to the reception of all varieties of sounds.

When this apparatus was put in operation a sound of peculiar quality, not unlike that of the human voice when in great distress, proceeded from the receiver. By altering the tension of the spring in various ways he was able to imitate many different sounds involving the vowels only, and succeeded, among other things, in producing a groan with all its inflections in the greatest perfection. Up to the time of making this experiment Mr. Gray had associated in his mind, in connection with transmission of spoken words, a complicated mechanism. This experiment produced an entire change in his views, and he came to the conclusion that in solving the problem of transmitting spoken words it was not necessary to consider the mechanism of the vocal organs at all, but simply the physical results produced in the atmosphere by them, and all that was necessary, therefore, was to devise a transmitter that would

reproduce electrically an exact copy of the atmospheric vibrations produced by any sound whatever.

During a visit in Milwaukee at this time, Mr. Gray saw for the first time a toy called the lovers' telegraph, consisting of a membrane stretched over the end of a tube, and having a thread attached to the center, the other end of which was attached to a similar membrane. The fact that spoken words were distinctly transmitted by the longitudinal vibrations of the thread from one membrane to the other confirmed the idea that he had formed a year previous; and it immediately solved in his mind the problem of making a transmitter that would copy electrically the physical vibrations of the air produced by articulate sounds. He determined to put this into practical shape, and file it in the records of the Patent-Office. He accordingly put his speaking telephone into the form of drawings and specifications, and filed them in the United States Patent-Office, February 14, 1876. In his specification he states that he has invented a new art of telegraphically transmitting vocal sounds whereby the tones of the human voice can be transmitted through a telegraphic circuit, and reproduced at the receiving end of the line, so that actual conversations can be carried on by persons at long distances apart, and that he has devised an instrument capable of vibrating responsively to all the tones of the human voice, and by which they are rendered audible.

His method of providing an apparatus capable of responding to the various tones of the human voice was a diaphragm stretched across one end of a chamber, carrying an apparatus for producing fluctuations in the potential of the electric current, and consequently varying in its power. The vibrations thus imparted were to be transmitted through an electric circuit to the receiving station, in which circuit was included an electro-magnet of ordinary construction, acting upon a diaphragm to which was attached a piece of soft iron, and which diaphragm was stretched across a receiving vocalizing chamber, similar to the corresponding vocalizing chamber at the transmitting end.

This is the first description on record of an articulating telephone which transmits and reproduces the spoken words of the human voice at a distance by means of electricity, and instruments constructed in exact accordance with Mr. Gray's drawings and specifications, filed at the time above stated, are good articulating telephones. Moreover, Mr. Gray's method of producing articulate speech by varying the resistance of a battery current is much more effective than that of Professor Bell subsequently invented, which depends upon magneto-induction currents generated by the action of the voice, as is fully proved by the great superiority of Edison's carbon telephone, which is based upon this principle.

CORRESPONDENCE.

DR. LARDNER AND TRANSATLANTIC STEAM-NAVIGATION.

To the Editors of the Popular Science Monthly.

GENTLEMEN: In your December number, under the heading "Editor's Table," you Socratically repeat the statement that Dr. Lardner declared that steam-navigation across the Atlantic was impracticable. This statement you will find on examination to be incorrect; and, as I consider that your publication is well adapted to make known the facts in the case, I transmit them to you, in the hope that you will publish them, and so contribute to their becoming generally known. Scientific men, as necessarily lovers of truth and exactness, can not but desire that such a statement should not be continued unless founded on the basis to which it pretends. General readers will soon adopt the correction of the error if properly laid before them, notwithstanding a possible bias toward enjoying what they at present regard as a disproved fallacy advanced by a very able man. The facts are as follows:

In 1828 there was published in New York, by Elam Bliss, 128 Broadway, an edition of Dr. Lardner's "Popular Lectures on the Steam-Engine, . . . with additions by James Renwick, Professor of Natural Experimental Philosophy and Chemistry in Columbia College, New York." Mr. Renwick in his preface says: "A few additions have been considered necessary. . . . They may be distinguished from the original paragraphs of the text from their being marked by letters instead of numbers, and their having the initials A. E. subscribed to each of them."

At page 157 of the work I refer to, the eleventh lecture of Dr. Lardner will be found to commence with this proposition: "One of the most interesting and important uses of the steam-engine is its application to *nautical purposes*. There are *various ways* in which this machine may be used in propelling a vessel through *the deep*; but that which is *now universally adopted* is by giving, through its means, rotation to paddle-wheels placed at the side of the vessel." And Dr. Lardner further on says: "In 1812 steam-vessels were first introduced upon the Clyde, and since that period steam-navigation has rapidly extended, so that at present there is scarcely a part of the civilized globe to which it has not found its way. *The Atlantic and Pacific Oceans* have been traversed by its powers, and if the *prolific results* of human invention should suggest means of diminishing the consumption of fuel, or obtaining a supply of heat from materials sufficiently small and light, *it would be hard to assign limits to the powers of this most wonderful agent.*"

Now, we have here from Dr. Lardner the broadest statement possible from so careful and exact a man, that he rather expected "the prolific results of human invention," not only to render "steam-navigation across the Atlantic" practicable, but to carry the uses of steam so far that he would not venture to assign limits to them. And the whole of his eleventh lecture is in this sense and tone.

But Mr. Renwick, at page 167 of the quoted work, has this note:

"(*k.*) The steam-engine may compete successfully with the wind as a propeller of vessels, whenever certainty of conveyance becomes important, as in the case of passage-boats upon lakes and rivers. But *there are cases where steam becomes inapplicable to navigation. Upon the open ocean*, although the safety of steamships has been fully tested, *the vast quantity of fuel necessary in a long passage will prevent its use* in distant voyages, and it is besides far less economic than the propulsion by means of sails. (A. E.)"

I have italicized a few of the more important words in these extracts, which show that it was not Dr. Lardner but Mr. Renwick who has to take the responsibility of having said that "steam-navigation across the Atlantic was impracticable."

Respectfully,

JAMES BURNS, M. D.

NEW ORLEANS, *December* 7, 1878.

INSECTS AND COLORED FLOWERS.

To the Editors of the Popular Science Monthly.

DURING the summer I have spent much of my time in a porch surrounded by petunias and morning-glories, of all shades of color from white to bright purple and dark violet. I first observed that the colored petunias were torn to pieces every day before noon, while the white or pale ones escaped almost uninjured. I soon discovered that the bees and butterflies were the mischief-makers, and that the damage was done with their sharp claws in struggling to get to the bottom of the flower-cup. I kept a close watch down to the present day—when the bees and butterflies are gone, and a few blossoms still remain, never molested—and

my first impressions have been fully confirmed. In every variety of situation and circumstances the white petunias have been neglected for the colored, in exact proportion to the intensity and vividness of color; and the same I found to be true, in a less degree, as regards the deep and pale morning-glories. I have called the attention of others to the facts, and proved that the preference of the insects is determined by color alone. If there was any difference whatever in sweetness or fragrance, it was in favor of the rejected white flowers.

Yours respectfully,

THOMAS D. LILLY.

KENT'S STORE, FLUVANNA COUNTY, VA., *October* 23, 1878.

AN AMERICAN HAIRY TORTOISE.

To the Editors of the Popular Science Monthly.

NOTICING your interesting extract, from "Land and Water," concerning the hairy tortoise, I take the liberty of mentioning a similar species found, to my knowledge, in the lakes of this valley. Its description tallies almost exactly with that of the Chinese variety, except perhaps in size. It is about three inches in length by two and a half in breadth, is very closely covered by its shell; the calipee is not hinged, and out of some dozen specimens examined by me not one was without the coat of water-grass. Its habitat is the bottom of shallow lakes and ponds, and near the submerged roots of trees, where it is often caught with the hook. It has a remarkably fetid odor. The grassy or confervoid covering is not of any very great length, generally about one half or three fourths of an inch. It is, I believe, an undescribed species, though Mr. Agassiz may have had a specimen among his collection of tortoises from the Mississippi Valley.

Very respectfully,

J. F. BATTAILE.

YAZOO CITY, MISSISSIPPI, *December* 8, 1878.

EDITOR'S TABLE.

COPYRIGHT AND MORALITY.

THE recent fluttering among American publishers caused by the discovery that Canadian enterprise threatens to come into successful rivalry with them, even in their own home market, is sufficiently amusing. The violation of the rights of foreign authors has been hitherto excused on the ground that it was necessary to the promotion of American popular education and indispensable to the intelligence of the country. Authors and publishers, we have been emphatically told, are by no means the main parties to be considered in this matter; both must be subordinated to the requirements of cheap literature for the reading public of the United States. This sounds patriotic and disinterested, and we might almost be persuaded to assent and admire, were it not for the odd circumstance that those who talk loudest in this strain seem to have been the most successful in feathering their own nests at the expense of the dear people whose interests they have so much at heart. The American publisher has been virtually saying to Jonathan and his family, who, it is presumed, were intensely hungering for knowledge, "By not paying the foreign author I am able to provide you with his productions many times cheaper than you could otherwise get them"; and it has been agreed that it was a very nice arrangement, highly favorable to American intelligence, which it might be a national disaster to disturb. But when the Canadian publisher offers to join in this noble philanthropic work of educating Jonathan and his family by cheap literature, we are surprised to observe that he gets the cold shoulder. He says to Jonathan and his family, "By not paying *American authors* I can furnish you with their productions many times cheaper than you can otherwise get them," and this he is proceeding to do by means of the post-office. But, instead of welcoming this efficient coöperation of the Canadian publishers for cultivating and illuminating the American mind, our publishers are quite disgusted, and say this thing must be stopped, which simply shows what

former pretensions have been worth. This northern gust will blow the dust away from many people's eyes.

But the American people ought not to have waited for this. It should have been settled on grounds of justice for the benefit of the national character. It is a serious question and a plain one—not easy to adjust, but still wholly practicable. It is one of those palpable matters in which where there is a will there is certain to be found a way. One of the worst things about it is that our practice shows to the world the low and disgraceful state of American morality. We have published the evidence of Huxley, Tyndall, and Spencer before the English Commission on Copyright, and every one who has read it will be struck by the clear and elevated ethical tone that pervades it. These men are thought by many to be very bad, but they are men who know what is right and believe in it and maintain it unflinchingly. Has the occupant of one American pulpit ever been known to call attention to this great national disgrace? International copyright is one of those questions that measure the degree of civilization. It indicates the high-water mark of the public conscience, the strength of the sense of justice, and how far it is overborne by the dictates of self-interest. It is a case in which wrong may be perpetrated with apparent impunity. More obtrusive questions which arise between people of different countries are liable to be complicated with fear, and justice is often extorted by a dread of the consequences of withholding it, rather than by the simple force of the conviction of right. But authors can't fight for their rights, nor will governments protect them by the force of arms. They must be content, therefore, to appeal to the moral sense and the sentiment of public honor. Military redress being out of the question, there remains only the resort to those civil agencies by which private rights are protected, and the vigor with which these act under the inspiration of public feeling tests the degree of civil progress or the condition of civilization. From this point of view the American Republic occupies the lowest place among the leading nations of the civilized world; and from the scorn of all honest men we can only escape by setting this matter right by some form of national action.

And the naked right of the case is palpable enough, though, from the obtuseness or indifference of the popular mind upon the subject, it can not be too frequently or too forcibly presented. What we have written elsewhere upon this point we now repeat, that it may have a more permanent record:

> The basis of an author's right of property in the book he makes is the same as the farmer's right in the wheat he raises. They are each the product of capital and labor. In one case capital is invested in land, implements, and stock; in the other it is invested in education, books, and suitable arrangements for literary life; while in both the product is the direct result of work done. The property in his work belongs to an author because there has been expense in its preparation, and because he has produced it by his immediate personal exertion. It is his to possess and to profit by its proceeds, by all the principles of justice which confer the ownership of any property. Questions may arise respecting acquired rights in literary property; but the original right of him who called it into existence by his own labor is clear and beyond question.
>
> It is often said that ideas are ethereal things, and belong to the spiritual world, and therefore can not become subject to ownership; that is, not being material property, they can not be real property. Others, again, curiously affirm that ideas may be property while yet in the thinker's mind, but cease to be so the moment they are sent forth and made useful to others; or that thought until expressed or published is the property of the thinker; when given to the world, like light, it is free to all. Now there is, of course, a profound difference between ideas and material commodities, but there is no such difference as is here implied. They are both products of human exertion. A sonnet is as much the result of bodily effort as a horseshoe. The author works with one material instrument, the

mechanic with another. The mechanic produces a hat, for example, by muscular action; and the author produces a book by brain action. In both cases the result represents vital power continuously expended; there has been exhaustion of the bodily energies in both cases alike. The blood represents the stock of vital power, and it is drawn upon in muscular action by the laborer and in cerebral action by the author. An author, too, has need for a large amount of blood to sustain his thinking. Physiologists tell us that the brain is more immediately dependent upon a copious supply of blood than any other part of the system. They say that, although the average brain is but one thirty-sixth the weight of the body, it demands one fifth of all the blood to keep it in working order. Ideas, then, are bodily products, and are at the expense of the blood; and the books containing them represent the life-forces and spent vitality of their authors. The elements of blood come from the market and have their price. Food and clothing, and the conveniences of living, are neither sent to authors from heaven, nor are they furnished gratuitously by government. Like other people, those who make books depend upon their labor to live, and they have an equal right with other people to the fruits of their labor.

Because ideas go forth in a subtile way, and are widely diffused, is no reason for regarding them as "free as the light," to be appropriated by anybody without paying their originators. If they are real and tangible enough to steal, they are capable of protection as property. It is in no sense a question of the form or quality of the things produced, for these are but accidents. If an author is to be robbed because his property is in an ethereal shape, why not rob the milkman because his property is in a liquid shape? Whether a man devote his powers to the elaboration of thought or the elaboration of matter is perfectly immaterial so far as concerns the question of his property right in the thing produced.

But we may go further and ask what property can have so sacred a right of recognition as that which is the product of man's highest faculties? The creations and constructions of the brain, as they have a far higher value to the world than the mere products of material manipulation, have also a more transcendent and inviolable claim to the protection of law. The hand is but the servant of the intellect. All knowledge, art, science, literature, discovery, invention, all that redeems man from barbarism and creates civilization, are but results of brain-exertion; and are we to discredit this noblest form of effort and outlaw the men whose lives have been consumed by it, while all the guards of legislation are thrown round the grosser forms of manipulative industry?

But it will be said all this is clear enough —why multiply wearisome commonplaces? Most true, and if our practice conformed to these professions, nothing would need to be said. But our practice is a direct contradiction to admitted and self-evident justice, and this is a state of things that can not be let alone. We affirm an author's right in his work, and then we proceed systematically to rob him of it. We are not only thieves, but thieves proclaiming in open day that we know ourselves to be so. To be sure, it is foreign authors whom we rob, but this does not affect the case, as the rights of private property are coextensive with civilization. It will hardly do to object to a "foreigner" when we appreciate his work so highly as to covet it and take it from him wrongfully. His case against us is clear enough. He says: "I have produced my book at much expense and with hard labor. I have created property in it; you want it, and in taking it you attest its value; you appropriate it to enrich yourselves without giving me a just equivalent; you steal it, and your Government protects you in it as if it were an innocent act." The logic is short and decisive, and it leaves this country in no very enviable position in the community of nations. Claiming to be civilized, enlightened, and even Christian, we practice an ethical code in this matter not higher than the standard of savages. Were our Government founded in usurpation and injustice, as we assert the European Governments to be, there would be a kind of consistency in maintaining this admitted wrong; but, while the monarchical countries have cleared themselves of all complicity in it, it is reserved for the people who boast of the "best government on earth," and who fill the world with their boasts about "human rights," to maintain an organized system of literary brigandage in which private rights are unscrupulously violated, and the highest products of man's exertion are the objects of indiscriminate plunder. It is a case, moreover, in which meanness is added to dishonesty. We take from its owner that which he is powerless to protect. We steal that which is confided to the public honor, and, as if this were not enough, we add hypocrisy to meanness by putting forth palavering pretexts for our action. There are

those who say the present state of things should not be meddled with, and rake up excuses for continuing it. It is bad to steal; it is despicable to steal from our benefactors who happen to be compelled to trust us; but more despicable still is the shameless Jesuitism by which an interested party will seek to defend it. When listening to the sneaking apologies that are put forward in extenuation of such conduct, we feel as if the mere common thief might rise in self-respecting wrath and kick the pettifogging poltroon out of his society.

Two things in relation to copyright may now be considered established as completely as anything can be established by the concurrent usage of the civilized world. By the declaration and the practice of all nations it has been settled, first, that an author has a right of property in his work, which government is bound to protect; and, second, that the public also has rights by which those of the author are restricted. There are extremists who maintain that an author's rights of property are absolute and perpetual, and other extremists who hold that there can be no such thing as exclusive property in ideas—just as there are those who maintain that "all property is robbery." Practical legislators may well assume that these conflicting views cancel each other, and may safely proceed to action on the basis of broad experience and the general agreement of nations.

LOCKYER ON THE CHEMICAL ELEMENTS.

MR. LOCKYER'S paper before the Royal Society on the compound nature of some of the so-called elements from the point of view of spectrum analysis has attracted the attention properly due to the eminence of the investigator in the latter field, but chemists will be slow to admit that the experimental aspect of the subject has been much altered by Mr. Lockyer's investigations. The *a-priori* grounds for believing that the so-called elements are not elementary were already strong. In fact the progress of chemistry had proved many substances to be compound which were previously ranked as elements, and left the list of simple bodies to consist of such only as have hitherto resisted analysis. It has long been believed that the relations among the atomic numbers furnish strong evidence of the compound nature of many substances classed as elementary; and spectrum analysis has served greatly to heighten this probability. When, a few years ago, Dr. Martineau made an assault upon the doctrine of evolution, one of his objections to it was that the universe started a long way ahead on the line of *heterogeneity* by its outfit of chemical elements; the implication being that these elements had to be separately created before evolution could begin. To this, Herbert Spencer cogently replied that the elements are not *known* to be elementary; that no intelligent chemist holds them to be absolutely so; and that many concurrent considerations compel the inference that they are compounded, and perhaps recompounded of a few and perhaps of a single primordial constituent. The bearings of spectroscopic research upon the question were thus stated: "Spectrum analysis yields results wholly irreconcilable with the assumption that the conventionally named simple substances are really simple. Each yields a spectrum having lines varying in number from two to eighty or more, every one of which implies the intercepting of ethereal undulations of a certain order, by something oscillating in unison or in harmony with them. Were iron absolutely elementary, it is not conceivable that its action could intercept ethereal undulations of eighty different orders: though it does not follow that its molecule contains as many separate atoms as there are lines in the spectrum, it must clearly be a complex molecule. Still more clearly is this general implication confirmed by facts furnished by nitrogen, the spectrum of which has two quite different sets of lines, and changes from one set to the other as the temperature is varied. The evidence thus gained points to the conclusion that, out of some primordial units, the so-called elements arise, by

compounding and recompounding; just as by the compounding and recompounding of so-called elements there arise oxides and acids and salts."

Mr. Lockyer aims to enforce these views by fresh illustrations. The following passages from an account of his paper written for the "London Times," by a chemist who heard it, will give a fair idea of its quality:

There are many facts and many trains of thought suggested by solar and stellar physics which point to the hypothesis that the elements themselves, or at all events some of them, are compound bodies. Thus it would appear that the hotter a star the more simple is its spectrum; for the brightest, and therefore probably the hottest stars, such as Sirius, furnish spectra showing only very thick hydrogen lines and a few very thin metallic lines, characteristic of elements of low atomic weight, while the cooler stars, such as our sun, are shown by their spectra to contain a much larger number of metallic elements than stars such as Sirius, but no non-metallic elements; and the coolest stars furnish fluted band-spectra characteristic of compounds of metallic with non-metallic elements and of non-metallic elements. These facts appear to meet with a simple explanation if it be supposed that as the temperature increases the compounds are first broken up into their constituent "elements," and that these "elements" then undergo dissociation or decomposition into "elements" of lower atomic weight. Mr. Lockyer next considers what will be the difference in the spectroscopic phenomena, supposing that A contains B as an impurity and as a constituent. In both cases A will have a spectrum of its own. B, however, if present as an impurity, will merely add its lines according to the amount present, as we have above explained; whereas if a constituent of A it will add its lines according to the extent to which A is decomposed and B is set at liberty. So that as the temperature increases the spectrum of A will fade if A be a compound body, whereas it will not fade if A be a true element. Moreover, if A be a compound body, the longest lines at one temperature will not be the longest at another. The paper chiefly deals with a discussion from this point of view of the spectrum of calcium, iron, hydrogen, and lithium, as observed at various temperatures; and it is shown that precisely the kind of change which is to be expected on the hypothesis of the non-elementary character of the elements has been found to take place. Thus each of the salts of calcium, so long as the temperature is below a certain point, has a definite spectrum of its own, but as the temperature is raised the spectrum of the salt gradually dies out and very fine lines due to the metal appear in the blue and violet portions of the spectrum. At the temperature of the electric arc the line in the blue is of great intensity, the violet H and K lines, as they are called, being still thin; in the sun the H and K lines are very thick, and the line in the blue is of less intensity than either, and much thinner than in the arc. Lastly, Dr. Huggins's magnificent star photographs show that both the H and K lines are present in the spectrum of α Aquilæ, the latter being, however, only about half the breadth of the former; but that in the spectrum of α Lyræ and Sirius, only the H line of calcium is present. Similar evidence that these different lines may represent different substances appears to be afforded by Professor Young's spectroscopic observations of solar storms, he having seen the H line injected into the chromosphere seventy-five times, the K line fifty times; but the blue line, which is the all-important line of calcium at the arc temperature, was only injected thrice. In the spectrum of iron two sets of three lines occur in the region between H and G, which are highly characteristic of this metal. On comparing photographs of the solar spectrum and of the spark taken between poles of iron, the relative intensity of these triplets is seen to be absolutely reversed; the lines barely visible in the spark photograph being among the most prominent in that of the solar spectrum, while the triplet, which is prominent in the spark photograph, is represented by lines not half so thick in the solar spectrum. Professor Young has observed during solar storms two very faint lines in the iron spectrum near G injected thirty times into the chromosphere, while one of the lines of the triplet was only injected twice. These facts, Mr. Lockyer contends, at once meet with a simple explanation if it be admitted that the lines are produced by the vibration of several distinct molecules.

The lithium spectrum exhibits a series of changes with a rise of temperature precisely analogous to those observed in the case of calcium.

In discussing the hydrogen spectrum, Mr. Lockyer adduces a number of most important and interesting facts and speculations. It is pointed out that the most refrangible line of hydrogen in the solar spectrum, *h*, is only seen in laboratory ex-

periments when a very high temperature is employed, and that it was absent from the solar protuberances during the eclipse of 1875, although the other lines of hydrogen were photographed. This line also is coincident with the strongest line of indium as already recorded by Thalén, and may be photographed by volatilizing indium in the electric arc, whereas palladium charged with hydrogen furnishes a photograph in which none of the hydrogen lines are visible. By employing a very feeble spark at a very low pressure the F line of hydrogen in the green is obtained without the blue and red lines which are seen when a stronger spark is used, so that alterations undoubtedly take place in the spectrum of hydrogen similar to those observed in the case of calcium. In concluding this portion of his paper Mr. Lockyer states that he has obtained evidence leading to the conclusion that the substance giving the non-reversed line in the chromosphere, which has been termed *helium*, and not previously identified with any known form of matter, and also the substance giving the 1,474 or coronal line, are really other forms of hydrogen, the one more simple than that which gives the *h* line alone, the other more complex than that which gives the F line alone.

There can be no question that the facts brought forward by Mr. Lockyer are of the highest importance and value, and that they will have much influence on the further development of spectrum analysis, to which he has already so largely contributed. But his arguments are of a character so totally different from those ordinarily dealt with by chemists that they will hesitate for the present to regard them as proof of the decomposition of the elements until either they are assured by competent physicists that they can not be explained by any other equally simple and probable hypothesis, or until what Mr. Lockyer has foreshadowed as taking place to such an extent in other worlds has been realized beyond question or cavil in our own laboratories.

LITERARY NOTICES.

THE COMMONWEALTH RECONSTRUCTED. By CHARLES C. P. CLARK, M. D. New York: A. S. Barnes & Co. 1878. Pp. 216. Price, $1.50.

THIS is a suggestive work on the philosophy of American politics, made up of two parts logically related but very dissimilar in character. The first half of the book is devoted to an examination of the tendencies of our political system, which are arraigned as, in their working, a disappointment and a failure. This portion of the work is important, as giving a large amount of information on the morbid anatomy and the diseased functions of the body politic. The facts are interesting and copious, but we regret that Dr. Clark has not guarded himself here against criticism by the more full and more careful citation of his authorities. For example, when he says, "Since 1870 more judges have been impeached, or have resigned to avoid impeachment, than in all our history before," or when he says, "The British Parliament, though it unites the powers and functions of all our separate State Legislatures and constitutional conventions, and manages half a hundred colonies and a fourth part of the population of the earth, does not pass as many laws annually as the State of New Jersey," we should like to be informed of the exact data on which these assertions rest. More care in this direction would have given to Dr. Clark's work a higher and more permanent value.

After pointing out, in his opening chapters, the numerous indications of our political degeneracy, the growing venality in public life, the increase of official crime, the augmenting incompetency of public men, the deeper corruption of parties, and the enormous increase of taxes, resulting from scandalous misgovernment—having, in fact, made out a strong and dark indictment against our political system, Dr. Clark then takes up the various causes and remedies that have been proposed for this bad state of things. He thinks the fault is not to be laid at the door of human nature, nor is the democratic principle to be blamed. The nation is not overgrown, our political evils are not the "aftermath of the late civil war," and our difficulties are chargeable neither to the Democratic nor the Republican party. They are such, moreover, as can not be rectified and removed by any of the usual expedients of reform, such as constitutional amendments, minority representation, cumulative voting, female suffrage, outlawry of the lobby, commissions of investigation, nonpartisan organizations, religion, moral renovation, education, or civil-service reform. The writer goes rapidly over this field, showing the weak places and the general

futility of these various remedial measures, and sums up that there is no hope in them, as follows:

> But these and many other various explanations and remedies for our evil case, that have been heretofore offered to the anxious inquiries of the people, hardly deserve so much attention. The explanations are incompetent and the remedies nugatory. He who expects to see statesmanship and fidelity to the public interests restored in city, State, or nation by civil-service reform, the restraint of special legislation, long presidential terms or short; a new settlement of functions among aldermen, commissioners, and mayors; the election or the appointment of judges; closer investigations or severer punishments; an educational test, a religious test, or a property test; or any other the like petty and partial devices, would expect to cure the yellow fever by changing a man's shirt.

This is strongly put, but we are inclined to think that the author is a good deal more than half right.

Dr. Clark thus discredits all the nostrums offered by the political doctors to cure the diseases of the body politic; but to what end? Not to confirm the conclusion, made infinitely probable by his own sweeping logic, that the case is not one to be cured by nostrums at all; but, strange to say, the other political medicine-men are dismissed, that our doctor may try a new nostrum of his own. His panacea *may* be effectual—it has not been tried—but we are sorry to note that it is put upon the usual quackish ground at the outset. Every sovereign cure assumes unity of cause in all diseases—"all maladies come from impurity of the blood, you know; here is something to purify the blood." Dr. Clark says, "The political rot in all the larger spheres of government is identical and pervading; it must own some single cause as dominating as gravity itself; and it must find a single cure."

The true root of our political difficulties is assumed to be the present organization of politics, represented chiefly by the caucus system, the result of which is, that "*at the dictate of leaders whom we have not chosen, we vote for candidates whom we do not know, to discharge duties that we can not understand.*" The remedy proposed is embodied in the proposition that popular elections work well in small and ill in large constituencies. The general purity of town and village politics is contrasted with the general corruption of municipal, State, and national politics, and the cause of this is alleged to be that, in the former case, the citizen knows who and what he is voting for, while in the latter case he knows little or nothing of either. There is therefore required a new method of elections—a reconstruction of the commonwealth by which the voter shall commit to competent men, whom he knows, the function of appointing all higher officials in the larger spheres of political action. The sovereign citizen is, in fact, to recognize his incompetency to deal with general politics, to abdicate his vote, on State and national questions, and choose those whom he thinks better able to do all this business for him. Dr. Clark does not propose to dispense with caucus and organization, but that the people shall take them out of the hands of the politician and operate them themselves. He says:

> The people must turn over the prerogative of choosing Governors and Legislatures, now nominally exercised at the ballot-box, to representative delegates. In the business of all large constituencies, the caucus and convention must be substituted for the polls. Thus only can the function of the voter be accommodated to his intelligence; and thus only, the shadow of power discarded, can we secure its substance.

For the details of his plan, the interested reader must be referred to Dr. Clark's work, where they are fully elaborated. Looking into it with some care, we have much the same opinion of it that its author very plainly expresses of other devices for the renovation of our politics and the salvation of the country. None the less we heartily commend his book to political readers, who will find much in it worthy of serious consideration.

RESEARCHES INTO THE EARLY HISTORY OF MANKIND AND THE DEVELOPMENT OF CIVILIZATION. By EDWARD B. TYLOR, D. C. L., LL. D., F. R. S. New York: Henry Holt & Co. 1878. Pp. 388. Price, $3.50.

THIS volume consists of a series of somewhat miscellaneous essays bearing upon the early history of man. In the view of the author, while there are great masses of materials already at hand for working out the subject, the time for writing a systematic treatise does not yet seem to have come. For civilization, being a process of long and

complex growth, can only be thoroughly understood when studied through its entire range; as the past is continually needed to explain the present, and the whole to explain a part. The matters here discussed have been chosen, not so much for their absolute importance as because, while they are among the easiest and most inviting parts of the subject, it is possible so to work them as to bring into view certain general lines of argument which apply not only to them, but also to the more complex and difficult problems involved in a complete treatise on the history of civilization.

The book contains essays upon "Gesture Language"; "Word Language"; "Picture-Writing and Word-Writing"; "Images and Names"; "Growth and Decline of Culture"; "The Stone Age, Past and Present"; "Fire, Cooking, and Vessels"; "Some Remarkable Customs"; "Historical Traditions and Myths of Observation"; "Geographical Distribution of Myths"; and "Concluding Remarks." One or two extracts from the last chapter will give the reader an idea of the spirit in which the inquiry is pursued. The author says:

> The facts collected seem to favor the view that the wide differences in the civilization and mental state of the various races of mankind are rather differences of development than of origin, rather of degree than of kind. . . . The state of things which is found is not indeed that one race does or knows exactly what another race does or knows, but that similar stages of development recur in different times and places. There is reason to infer that our ancestors in remote times made fire with a machine much like that of the modern Esquimaux, and at a far later date they used the bow and arrow, as so many savage tribes do still. The foregoing chapters, treating of the history of some early arts, of the practice of sorcery, of curious customs and superstitions, are indeed full of instances of the recurrence of like phenomena in the remotest regions of the world. We might reasonably expect that men of like minds, when placed under widely different circumstances of country, climate, vegetable and animal life, etc., should develop very various phenomena of civilization, and we even know by evidence that they actually do so; but, nevertheless, it strikingly illustrates the extent of mental uniformity among mankind to notice that it is really difficult to find among a list of twenty items of art or knowledge, custom or superstition, taken at random from a description of any uncivilized race, a single one to which something closely analogous may not be found elsewhere among some other race, unlike the first in physical characters and living thousands of miles off. It is taking a somewhat extreme case to put the Australians to such a test, for they are, perhaps, the most peculiar of the lower varieties of man, yet, among the arts, beliefs, and customs found among their tribes, there are comparatively few that can not be matched elsewhere. They raise scars on their bodies like African tribes; they circumcise like the Jews and Arabs; they bar marriage in the female line like the Iroquois; they drop out of their language the names of plants and animals which have been used as the personal names of dead men and make new words to serve instead, like the Abipones of South America; they bewitch their enemies with locks of hair; and pretend to cure the sick by sucking out stones through their skin, as is done in so many other regions. It is true that among their weapons they have one of very marked peculiarity, the boomerang, but the rest of their armory are but varieties of instruments common elsewhere. They show but few exceptions to the general rule that whatever is found in one place in the world may be matched more or less closely elsewhere.

The author believes that "the history of mankind has been, on the whole, a history of progress." Some facts are quoted which bear on the possible degeneracy of savage tribes when driven out into the desert, or otherwise reduced to destitution, or losing their old arts in the presence of a higher civilization; but there seems ground for thinking that such degeneration has been rather of a local than of a general character, and has affected the fortunes of particular tribes rather than those of the world at large.

MANUAL OF INTRODUCTORY CHEMICAL PRACTICE. For the Use of Students in Colleges and High-Schools. By GEORGE C. CALDWELL, S. B., Ph. D., and ABRAM A. BRENEMAN, S. B., Chemical Professors in Cornell University. Second edition, revised and corrected. New York: D. Van Nostrand. 1878. Price, $1.50.

IN its earliest form this work consisted of detached sheets for the use of students of chemical practice. Corrected by trial, they were published in book form two years ago. We have now the second edition, in which the authors, guided by their larger experience, have been able better to adapt the work to the average capacity of students. Some experiments have been modified or rejected and others introduced, and another section added to the introduction for the help of teachers. The experiments are chosen to illustrate principles, and in the performance of the experiment the stu-

dent is left to observe and describe it and trace its connection with the principle it illustrates. In this work precision and conciseness of statement are required, and the reports are subjected to critical examination by the teacher. With each copy of the book there goes a separate slip stating how much apparatus and chemicals are needed for every ten students taking this course of practice.

While in its main features the manual is introductory to a more extended course, it may still be used with profit by students who have no time or opportunity for subsequent study.

LESSONS IN ELEMENTARY CHEMISTRY: Inorganic and Organic. By HENRY E. ROSCOE, B. A., F. R. S., Professor of Chemistry in Owens College, Manchester. New edition. New York: Macmillan & Co., 1878. Price, $1.50.

THIS text-book, which was first published in 1869, conforms to the metric system of weights and measures, and the centigrade thermometric scale. The most important facts and principles of modern chemistry are so presented as to give the pupil exactitude of knowledge, without which science in schools is worthless. The work was revised in 1875, and important changes were made in the organic portion, while the whole book was brought up to the level of the science of the day. The present edition, among other alterations and additions, adopts, for the combining weights of the elements, numbers derived from Sta's accurate experiments, oxygen being taken at 15·96 instead of 16. In ordinary calculations, however, the older numbers, for the sake of simplicity, are still employed.

CHRIST'S WORDS AS RELATED TO SCIENCE, LAW, GOVERNMENT, HISTORY, PHILOSOPHY, RELIGION, AND UNIVERSAL HUMAN EXPERIENCE. By Prof. J. B. TURNER, of Jacksonville, Illinois, author of "Three Great Races of Men," "Essays on Meteorology," "Industrial Education," etc. Springfield, Illinois: H. W. Rokker. Pp. 425. Price, $2.50.

THIS is an able and vigorous work by an earnest believer in the religion of Christ, the object of which is to cleave through the body of dogmatic theology that has been accumulating for centuries, and get at those simple teachings that are embodied in the words of the founder of the Christian faith. "What is it that Christ really taught, and that constitutes the essence of his religion?" is the problem that Prof. Turner puts before him and undertakes to solve. The task he has ventured upon opens a very broad field of inquiry, embracing various departments of knowledge. Prof. Turner does not claim to be an expert in all these branches of learning, but only to have considered them with reference to one controlling idea—how far they have been employed to obscure the elementary inculcations of the Master. The author is both a firm believer and a vehement doubter. He says he "does not believe that all or most of the current ideas of either religion or science, of philosophy, law, or history, are true, or in accord with the teachings of Christ. It will be forever impossible to harmonize such a medley of false assumptions with each other or with the facts of being, either natural or spiritual." It being thus assumed as out of the question to bring the chaotic schemes of belief into reconciliation, the sincere Christian has nothing left but to find out for himself that which is essential in his religion, and Prof. Turner avows no other object than to aid him in this work.

The author remarks: "What Christ actually said may be one thing; what the world has been catechised or thumb-screwed into the belief that he said may be quite another." How natural it is for the meanings of Scripture to have been distorted in the long ages of ignorance and prejudice, during which they have been a matter of conflict, is well illustrated by Prof. Turner in an example which everybody can understand. He says: "If in half a century our national Constitution, written in our native tongue, consecrated to the broadest liberty, could be perverted so that union, fraternity, and justice, were synonymous with the right of domination of white over colored men; and if our Legislatures, our courts, our army and navy, our literature, schools, and churches, our very psalms and prayers, could be marshaled and used for the defense of one of the most infamous forms of slavery the world has ever seen, what may not have been done during the ages of barbaric ignorance, with the records

of the Bible existing only in manuscript, and written in dead Hebrew and Greek?"

From Prof. Turner's chapter on "Miracle and Prayer" we quote the following passages, which illustrate his view of that subject:

We have a cow that is, in her way, a great philosopher, and somewhat of a divine. She has attained such adroitness that she can handle all the hasps and latches, and open all the gates and barn-doors. She is clearly of the opinion that whatever lies beyond her capacity in that line must be miraculous; and when we take a key out of our pocket, and put it in a padlock, and open a door that she has tried in vain to open, she cocks her ears forward, opens her eyes, and says as plainly as she can: "Well, now, that is clearly miraculous; a manifest interference with the laws of Nature." And this is very good cow-philosophy and cow-theology; but will it do for human beings? . . .

No one of the gospel narrators ever intimates that Jesus's works were either a violation or a suspension of, or even an interference with, any law of Nature. All this is our own "cow-philosophy" and "cow-divinity." It neither came from Christ nor his apostles. They spoke of them as "*signs*" and "*wonders*," "*mighty works*"; as acts that were "significant," "strange and unusual," and implying "power." In the only three ultimate forms of being or existence known to us, matter, force, and spirit, or voluntary being of some sort, the last is the only one from which any new force or cause can even seem to originate.

Prof. Turner writes with great vigor and force, though we think with some verbal redundancy, and is mainly intent upon making himself understood. He is inclined to consider that there is a good deal of credulity on the part of scientific men, and he is not very mealy-mouthed in his statement of this opinion. The following passage is evidently for the benefit of Prof. Tyndall:

Contrast, now, the ontology, or scheme of being and destiny, implied in all Christ's teachings and works, with that implied in the dogmatism of those scientists who find "all potency in matter," beginning with their universe of star-dust and incandescent gas, without known cause, solidifying itself into the solid worlds, generating protoplasms, bioplasms, and cells, and even correlated sexes, out of dead matter, with no supervising intelligence: the molten mass of earth cooling down so slowly as to admit of ages of tropical life at the poles; and anon for ages taken with such a congestive chill that eternal ice and glaciers shrouded it quite down to the tropics; then a fever set in again, and it warmed up to its present condition, full of literal hell-fire within, eternally belching forth in all its volcanoes—all living things made out of the same original protoplasms, more handy to the modern scientist than was the dust of the earth, or Adam's extra rib, to the old Jews, but made without any maker. All things at last, in prophetic vision, to turn to ice again; all being, even the sun himself, is to freeze to death; a universe of being born without God, born at first of hell-fire, nursed on protoplasm without any nurse, and consigned at last to eternal death by frost, with still no God to breathe the breath of life into it forever more; or it may take a notion to explode again into gas and star-dust, to run its perpetual rounds, with alternate creations by hell-fire and damnations by frost, through all eternity to come! What an origin! What a destiny! What logic! What shocking assumption at every step! and what infinite dogmatism in every conclusion! What aimless and senseless credulity! Jonah and his whale, Joshua and his sun, Noah and his ark, Moses with all his snakes, and frogs, and lice, and murrains, and deaths, are totally eclipsed by these modern dealers in scientific miracles.

THE AMERICAN QUARTERLY MICROSCOPICAL JOURNAL. Edited by ROMYN HITCHCOCK. Devoted to the Interests of Microscopical Study in all Branches of Science; with which is also published the Transactions of the New York Microscopical Society. Vol. I., No. 1. Published by Hitchcock & Wall, 150 Nassau Street, New York. Price $3 a year, or 75 cents per copy.

THIS is a compact, neatly printed, and beautifully illustrated journal of 96 pages, intended as an aid to professional and amateur microscopists in the promotion and diffusion of the results of research. Of the microscope and the functions of a microscopical journal the editor says:

To the student of natural science the microscope is, and always will be, a mere tool. Microscopy, as a special science, has very little claim for existence. In so far as a certain familiarity with the instrument and training in the proper management of the light and accessories are necessary to enable one to use the instrument, it may be called a science. We would detract nothing from the merits of those who are expert in securing the most perfect performance of an objective. Still, as a matter of fact, and plain facts should not give offense to any one, we must admit that the great value of the microscope as a means of investigation lies in the aid it gives to almost every branch of science. This leads us to a statement of what, in our opinion, a microscopical journal should be. Recognizing the value of microscopical study in the various branches of natural science, such a journal should aim to publish the results of research carried on with the microscope in every department.

Accordingly, besides articles relating to the structure and improvement of the mi-

croscope itself, we find in the first number the science of zoölogy represented by papers on "The Sting of the Honey-Bee," and "On the Structure of Blood-Corpuscles"; botany by "Descriptions of New Species of Diatoms," and "On the Spore-Formation of the Mesocarpeæ"; the arts, by "The Microscopical Examination of the Fibres"; and in the next number we are promised among others an article on microscopical geology, and a study of a case of tubercular meningitis; while the subject of foods will receive attention in a paper on the microscopical characters of natural and artificial butter. From this enumeration it will be seen that the editor intends to cover a wide range of topics; and, while probably in the majority of cases the discussions will be more or less technical, many of them will also possess both popular and practical interest. The projectors of the enterprise are entitled to great credit, not only for the handsome magazine they have made, but for their courage in entering a field already the scene of many failures, and we would ask for them, not the sympathy, but the cordial support, of all who are interested in the progress of science.

SCIENCE NEWS. Edited by ERNEST INGERSOLL and WILLIAM C. WICKOFF, of New York, and published fortnightly by S. E. Cassino, Salem, Mass. Subscription price, $2 a year.

THE main object of this new periodical, as indicated by its title and expressly announced in the prospectus, is the prompt publication of scientific news, and, judging from the four numbers received, the editors are fulfilling their promise in a very satisfactory way. Each number is to contain not less than sixteen pages of matter, exclusive of advertisements, freely illustrated and presented in a style that may be readily understood by the average reader. The type is good, the price is reasonable, the editors are wide awake, and, with the advantages of thorough scientific training and long previous experience on scientific journals, are well adapted to the work—conditions certainly favorable to the production of a good magazine. Yet in one respect we think the new journal might be improved. There is news enough of a scientific character, and of both special and general interest, to more than fill such a periodical, even if it contained double the present number of pages, and to our minds this, in the form of paragraphs as brief as an intelligible statement will permit, might be profitably substituted for the more elaborate essays which now occupy the earlier pages of the periodical.

THIRD ANNUAL REPORT OF THE JOHNS HOPKINS UNIVERSITY. Baltimore: Murphy print, 1878. Pp. 60.

MR. D. C. GILMAN, President of the Johns Hopkins University, in this report, strives to draw the line of distinction between the *college* and the *university*. The university is designed to give to those who have already received a college training or its equivalent more advanced and special instruction. To this end it must possess ample libraries, laboratories, and apparatus. Then, the holders of professorial chairs in a university must be expected and encouraged to advance by positive research the sciences to which they are devoted. For though, primarily, instruction is the duty of the professor in a university, as it is in a college, the difference of intellectual maturity between the students of the two institutions involves a difference in the respective demands of each upon the professor: university students should be so mature as to exact from their teachers the most advanced instruction, and even to quicken and inspire by their appreciative responses the new investigations which their professors undertake. An interesting feature of this report is a statement by Professor Ira Remsen, of the nature of the work done in the three scientific laboratories which form part of the Johns Hopkins University—viz., the biological laboratory, the physical laboratory, and the chemical laboratory.

NOTES ON A COLLECTION FROM THE ANCIENT CEMETERY OF CHACOTA BAY, PERU. By J. H. BLAKE. With Illustrations. From "Report of Peabody Museum of Archæology and Ethnology," 1878. Pp. 28.

THE interesting collection here partly described comprises mummies, utensils, weapons, ornaments, etc. What race of people it was that buried their dead in this ancient cemetery it is impossible even to conjecture. The present Indian inhabitants of the locality claim no relationship with them.

THE TELEGRAPH IN AMERICA: Its Founders, Promoters and Noted Men. By JAMES D. REID. New York: Derby Brothers. 1879. Pp. 850. Price, cloth, $6.

WHEN in June, 1871, a strong representation of the telegraph interest in America was assembled in New York City to attend the ceremony of unveiling the statue of Professor S. F. B. Morse, in the Central Park, an earnest desire was expressed by many to have the occasion and the man appropriately commemorated in a volume. The task of composing this memorial volume was imposed upon Mr. Reid, and the completed work is now published: but instead of its being simply a monument to the memory of Professor Morse, the work has been expanded to the proportions of a history of telegraphy in America.

In accordance with the original intention of the author, the volume contains a pretty full biography of Professor Morse, with an account of the progress of electrical science down to the year 1832, when he first conceived his idea of a recording electrical telegraph. Mr. Reid was an intimate friend of Morse, and reverently cherishes his memory; but in writing this account of his friend's researches and inventions, he exhibits no desire to slur the merits or to belittle the labors of other workers in the same field. The story of Morse's invention of the recording telegraph is told without rhetorical embellishment, but with the effectiveness of simple narrative. It was in the early part of October, 1832, and Morse was crossing the Atlantic on his way home from Europe, whither he had gone some three years before, to study the works of the great painters, for he was an artist before he turned his attention to telegraphy. One of his fellow travelers was Dr. Charles T. Jackson, of Boston, then profoundly interested in electro-magnetism, to which his attention had been directed by certain lectures which he had heard in Paris. In conversation with Morse he described in particular Ampère's brilliant experiments with the electro-magnet.

"The subject," writes Mr. Reid, "at once excited very general interest, into which Mr. Morse entered with awakened enthusiasm. Hitherto he had felt no other interest in electrical matters than that of a lively and excited curiosity. His early studies now enabled him to enter into the conversation with intelligent earnestness. Dr. Jackson had in his trunk, in the hold of the vessel, an electro-magnet, which he described, and during the conversation alluded to the length of wire in the coils. This led one of the company to inquire 'if the velocity of the electricity was retarded by the length of the wire.' A very pregnant thought lay in that inquiry, and the conversation became earnest and practical. Dr. Jackson replied that electricity passed instantaneously over any known length of wire. At this point Mr. Morse remarked, 'If the presence of electricity can be made visible in any part of the circuit, I see no reason why intelligence may not be transmitted instantaneously by electricity.'"

The author has had access to the artist's sketch-book, in which Morse at the time jotted down his alphabet scheme, and drew designs of various pieces of apparatus. These are reproduced in Mr. Reid's volume, and thus the reader is enabled to see Morse's system of telegraphy in its germ, so to speak. The author follows his own account of the "Birth of the Recording Telegraph," with the history of the invention composed by Morse himself in 1868, on the occasion of the International Exposition at Paris.

This "Morse Memorial" occupies the first one hundred pages of Mr. Reid's volume; the remainder is devoted to the "History of the Telegraph in America." The plan of this second part is an unfortunate one, comprising sketches of the rise and development of the different telegraph companies, with notices of their founders and promoters. This arrangement necessarily makes the work a congeries of mutually independent memoirs, each one valuable indeed in itself, but the effect of the whole must be to weary and confuse the reader's mind. Nevertheless, the work is one possessing permanent value, not as a "History," but rather as a collection of *mémoires pour servir*—of authentic materials which the philosophical historian will later digest and coördinate. It is safe to say that no future historian of the telegraph can afford to overlook the work done by Mr. Reid.

The book, in its mechanical execution, leaves nothing to be desired. It contains portraits, some in steel plate, others in

wood engraving, of many men of note connected with telegraphy in America, whether as inventors or as administrators.

WALKS IN LONDON. By AUGUSTUS J. C. HARE. 2 vols. in one. New York: Routledge & Sons. 1878. Pp. 1,020.

"IN these volumes," writes the author in the preface, "I believe all the objects of interest in London are described consecutively as they may be visited in excursions, taking Charing Cross as a center. The first volume is chiefly devoted to the city, the second to the West End and Westminster. . . . While endeavoring to make 'Walks in London' something more interesting than a guide-book, I have tried, especially in Westminster Abbey and the picture-galleries, to give such details as may suggest new lines of inquiry to those who care to linger and investigate."

SCIENCE OBSERVER (Monthly). Volume II., No. 3. Boston: Amateur Scientific Society. Post-Office Box 2,725. Pp. 8. Subscription, 50 cents per year.

WE observe with pleasure the continued success of this very meritorious little periodical. It appears to be particularly strong in the department of astronomy. The present number, for instance, has an elaborate article on "The Tides." This is the "leading article" of the number. The minor articles are on "The August Lyriads," "Mira Ceti," "Sun Spots," "The Intra-Mercurial Planet"; finally, we have "Ephemerides of Variable Stars."

SURVEY OF THE NORTHERN AND NORTHWESTERN LAKES AND THE MISSISSIPPI RIVER, in charge of C. B. COMSTOCK, Major of Engineers, and H. M. ADAMS, Captain of Engineers. With Charts. Washington: Government Printing-Office. 1877. Pp. 100.

THE work of this survey, during the year ending June 1, 1877, may be thus summed up: On Lake Erie the triangulation has been carried from Westfield, New York, to near Painesville, Ohio; the topography and hydrography have been carried from Ashtabula, Ohio, to Vermilion, Ohio; the latitudes and longitudes of sundry points have been determined; a line of levels has been run between Escanaba and Marquette, to determine the difference of levels between Lakes Michigan and Superior; four charts of Lake Michigan, one of the St. Lawrence, and one of the Detroit River have been completed; the survey of the Mississippi has been carried from five miles above Cairo to a point eight miles above Columbus, Kentucky.

GEOLOGICAL AND NATURAL HISTORY SURVEY OF MINNESOTA (1877). With Maps. Minneapolis: Johnson, Smith & Harrison print. Pp. 225.

THE first work undertaken by the officers connected with the Minnesota survey in the year 1877 was an attempt, and a successful one, to ascertain the causes of the foulness of the water in wells throughout the Red River Valley. It appears that, owing to the scarcity of building-stone in the valley, pine planks are used for curbing the wells, and to this cause is to be attributed the bad quality of the water, which in itself contains nothing that is unwholesome. Professor Winchell, State Geologist, with his associates, next examined localities in Wright County, where coal had been supposed to exist. At no point were Cretaceous beds seen *in situ*, though possibly they might be struck below the drift, in sinking a shaft. Preliminary reconnaissances were made into the counties of Goodhue and Morrison. The surveys of the following counties were completed, viz., Hennepin, Rock, Pipestone, and Rice.

THE MINERALS, ORES, ROCKS, AND FOSSILS IN THE PACIFIC COAST EXHIBIT AT THE PARIS EXPOSITION OF 1878. San Francisco: Bosqui & Co. print. 1878. Pp. 99.

WE have here a catalogue of the collection named above, preceded by an Introduction on the mineral resources of the Pacific States.

THE INDIAN QUESTION. Address by General POPE. Pp. 31.

THE author proposes to locate reservations for Indians far in the rear of advancing emigration, in populous, well-ordered districts, where no hostility to the Indian is felt. Thus surrounded by good influences, the Indian might, the author thinks, become civilized, and perhaps eventually absorbed by the superior race.

PUBLICATIONS RECEIVED.

Cooking-School Text-Book. By Juliet Corson. New York: Orange Judd Co. Pp. 240.

Outline of General Geology. By Theo. B. Comstock. Ithaca, New York: University Press. Pp. 82.

Coal: its History and Uses. Edited by Professor Thorpe. London: Macmillan & Co. 1878. Pp. 350. $4.

Red Eagle. By G. C. Eggleston. New York: Dodd, Mead & Co. 1878. Pp. 346.

Elements of Comparative Anatomy. By Carl Gegenbaur. London: Macmillan & Co. 1878. Pp. 645. $7.

The Bride of Gettysburg. By J. D. Hylton. Palmyra, New Jersey. 1878. Pp. 172.

The Reign of God *not* "the Reign of Law." By Thomas Scott Bacon. Baltimore: Turnbull Brothers. 1878. Pp. 400. $1.50.

Political Economy. By William Roscher. New York: Holt & Co. 1878. Two vols., pp. 464 and 465. $7.

The Labor Side of the Great Sugar Question. New York. 1878. Pp. 30.

Researches in Telephony. By Professor Dolbear. From "Proceedings of the American Academy of Science and Arts." Pp. 15.

Report on Life-Saving Apparatus. By Lieutenant D. A. Lyle, of the Ordnance Department. Washington: Government Printing-Office. 1878. Pp. 100, with numerous Plates.

The Temperaments. By Dr. D. H. Jacques. New York: S. R. Wells. Pp. 239. $1.50.

Transmission of Power by Compressed Air. By R. Zahner. New York: Van Nostrand. Pp. 133. 50 cents.

Mansill's Almanac of Planetary Meteorology for 1879. Rock Island, Illinois: R. Crampton. Pp. 52. 50 cents.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. IV., Nos. 2, 3, 4. Washington: Government Printing-Office. 1878.

Bulletin of the United States National Museum. Nos. 10 and 12. Washington: Government Printing-Office.

Bibliography of North American Invertebrate Paleontology. Pp. 119. Catalogue of Photographs of Indians. Pp. 122. Washington: Government Printing-Office.

Birds of the Colorado Valley. By Elliott Coues. Washington: Government Printing-Office. Pp. 842.

Drift from York Harbor. By G. Houghton. Boston: A. Williams & Co. 1879. Pp. 48.

Total Abstinence. By Dr. B. W. Richardson. London: Macmillan & Co. Pp 119.

Handbook of Alabama. By S. Berney. Pp. 333, with Map. Paper, $1.50.

Noxious and Beneficial Insects of Illinois. By Dr. C. Thomas. Springfield, Illinois: Lusk print. Pp. 290.

Reptiles and Batrachians of California, etc. By Dr. H. C. Yarrow. Pp. 23. Marine Fishes. By the same author. Pp. 7. Washington: Government Printing-Office.

Physiology of the Spermatozoa. By Dr. S. S. Herrick. Pp. 7.

Text-Books and Methods of Instruction in English. By J. M. Garnett. ("Educational Journal of Virginia.") Pp. 20.

Almanac for Use of Navigators for the Year 1879. Washington: Bureau of Navigation. Pp. 259. 50 cents.

Difficult Labor. By Dr. E. M. Hale. Pp. 94.

Native Wild Flowers and Ferns. By Thomas Meehan. Parts 13, 14, 15, 16. Boston: Prang & Co.

POPULAR MISCELLANY.

Reorganization of the Government Surveys.—We have already briefly stated the conclusions reached by the Committee of the National Academy of Sciences, appointed to consider what changes might be desired in the method of conducting the surveys of the Territories. We have since received an official copy of the Committee's report, and consider its subject matter of sufficient importance to justify a fuller abstract. The Committee interpreted the act of Congress directing their inquiry as applying only to surveys of the public domain, and hence did not take into consideration surveys or investigations which have for their objective point engineering works: such surveys, in the judgment of the Committee, should be conducted by the engineer corps of the army. The surveys which, in their opinion, were intended by the act to be inquired into were those popularly known as Wheeler's, Hayden's, Powell's, and the Land Surveys under the supervision of the Land-Office. Besides these, though not enumerated in the law, there is the Coast and Geodetic Survey. All the work done by these different corps may be classed under two heads: 1. Surveys of mensuration; 2. Surveys of geology and economic resources of the soil. The surveys of mensuration are at present conducted by five independent organizations, already named. There is no coördination between these five surveys, and their results show many contradictions. The geographical work of Wheeler's, or Hayden's, or Powell's survey is of little value for the parceling of land, while the land surveys are of correspondingly slight topographical and geographical value. The opinion of the Committee is, that "the Coast and Geodetic Survey is practically best prepared to execute the entire mensuration system required." But the Committee recommend that this survey be transferred from the Treasury to the Department of the Interior, and that in addition to its original field of work it should also assume the entire mensuration of the public domain.

The Geological Survey should have a separate organization. "To meet the requirements of existing laws in the disposition of the agricultural, mineral, pastoral, timber,

desert, and swamp lands, a thorough investigation and classification of acreage of the public domain are imperatively demanded. The Committee, therefore, recommends that Congress establish, under the Department of the Interior, an independent organization to be known as the United States Geological Survey, to be charged with the study of the geological structure and economic resources of the public domain." And the Committee recommends a discontinuance of the present Geographical and Geological Surveys and the present Land Surveys. "The effect of the above changes," says the Committee, "will be to maintain within the Interior Department three distinct organizatians: 1. The Coast and Interior Survey, whose function will embrace all questions of position and mensuration; 2. The United States Geological Survey, whose function will be the determination of all questions relating to the geological structure and natural resources of the public domain; 3. The Land-Office, controlling the disposition and sale of the public lands, including all questions of title and record. With this division should be secured a perfect coördination and coöperation between the three branches. The Land-Office should call upon the Coast and Interior Survey for all surveys and measurements required for the sale and disposition of land. The Land-Office should also call upon the United States Geological Survey for all information as to the value and classification of lands. The results of all the mensuration surveys, as soon as completed, should be immediately available for the Land-Office and for the Geological Survey, and for other branches of the Government as required. The Geological Survey should be authorized to execute local topographical surveys for special purposes, such, for instance, as the subterraneous surveys of mining districts and metallic deposits, etc."

Huxley on the Hand.—Professor Huxley chose for the subject of a recent lecture at the Workingmen's College, of which he is President, the human hand. He looked on the hand as not second in importance even to the brain itself. He pointed out the great diversity of operations for which man is dependent on the hand, and observed how it performs all its important functions by virtue of certain very simple facts in its form of construction. He referred to that famous work, Paley's "Natural Theology," and the argument which it enforces—that if a person were to find the whole machinery of a watch he must needs infer from the works of it that it *must have been intended* to serve a certain purpose. But Professor Huxley pointed out that, whatever the force of the argument of analogy in the case of the hand, it most assuredly does not apply in the sense in which it was used by Paley, because it can easily be shown that a man's hand was not put together in that way, but that it came about in quite a different manner. It was not a process in any way analogous to human means of construction, being, in fact, as different from the latter as the taking of a piece of iron and making it into an engine differs from taking it and letting it grow to an engine. This difference, he remarked, is highly important, as showing the danger of arguing from mere analogy—it shows that Paley's argument is not consonant with fact. Paley could not conceive that so complicated a structure as the human frame might, as a matter of fact, be developed or evolved by a purely natural operation.

Vehicles of Malaria.—Ague is commonly supposed to be due to the entrance into the system of a miasmatic organism. But no microscopist has ever seen this organism, neither can we account for the intermittency of the ague-paroxysms, nor can we say for certain through what medium it finds an entrance into the system. The majority of writers hold the opinion that *the air of marshes* is the sole cause of intermittent fever. But there exists strong evidence going to show that water, too, is a carrier of the poison. Take, for instance, two or three cases cited in the "Lancet"; and, first, the case recorded by Boudin, of three vessels sailing from Algiers to Marseilles, conveying eight hundred soldiers, who on shore had all been exposed to the same atmospheric conditions. Two of these vessels were supplied with good water, but the third with water from a marsh. The two former arrived at Marseilles without a sick man, but the third ship lost thirteen men, and had one hundred and twenty sick, nine-

ty-eight of whom were affected with malaria. Again, there is the outbreak of ague at Tilbury Fort in 1872, cited in Parkes's "Hygiene," where thirty-four men out of a garrison of one hundred and three were seized with ague, while the people at the railway station and the coast-guard men and their families just outside the fort entirely escaped. The troops had been supplied with water stored in tanks, collected from the rain-water of the roofs, while the people outside obtained theirs from a spring, the atmospheric conditions in both cases being identical.

The Werdermann Electric Light.—Mr. Edison's announcement of his success in solving the problem of adapting the electric light to domestic purposes has had the effect of bringing into public view a number of other contrivances for producing the same result. Among these, Werdermann's system appears to be perhaps the most promising; the following account of it we take from "Nature": "The principle of Werdermann's invention is that of keeping a small vertical pencil of carbon in contact with a large disk of the same material. In some earlier experiments he found that when he increased the sectional area of one carbon and reduced that of the other he produced an electrical light with the carbons in actual contact, a small arc appearing at the contact-point. The small carbon is a pencil three millimetres in diameter; the upper or negative carbon is a disk of two inches in diameter and an inch thick. The upper carbon is not consumed, so that the waste takes place only in the lower. In his lamp he places the disk uppermost with the pencil vertically beneath it, sliding up a metal tube which acts as a guide and contact. The pencil is kept in contact with the disk by means of chains attached to its lower extremity, passing over pulleys and down again to a counterweight of about one and a half pound. About three quarters of an inch of the lower carbon appears above the tube and is rendered incandescent by the passage of the current between it and the disk. This pencil is pointed, and retains its point all the time of burning. It is between this point and the disk that the small electric arc appears which gives the greater part of the light. In an exhibition of the system, ten lamps were shown in one circuit. The inventor believes that after further experiments he will be able to divide the current into fifty, one hundred, or even five hundred lights. Each lamp can be lighted and extinguished separately without affecting the others."

Dangers of Moldy Bread.—A singular case of poisoning from eating a pudding made in part of moldy bread is reported in the "Sanitary Record." The main facts of the case may be briefly stated as follows: The principal materials of the pudding consisted of scraps of bread left from making toast and sandwiches, and they had been about three weeks accumulating. To these scraps were added milk, eggs, sugar, currants, and nutmeg. The whole was baked in a very slow oven, and was subsequently eaten by the cook, the proprietor of the eating-house in which it was prepared, the children of the proprietor, and two other persons. All of these became violently ill, with symptoms of irritant poisoning. One of the children (aged three years) and one of the adults died. The necropsy of the body of the child caused the medical men to suspect poisoning, and accordingly the viscera, together with the remnant of the pudding, the materials used in making it, the matter vomited, etc., were sent to a chemical analyst, Mr. Alfred Allen, for examination. He made tests for several poisons, but without positive result. A puppy was fed with the pudding for two days without any poisonous effect. He was then led to look for ergot in the pudding, and was soon startled to find unquestionable evidence of its presence, as far as the chemical reactions went, though he was unable, with the aid of the microscope, to detect any actual ergot. From these facts Mr. Allen infers that the reactions hitherto supposed to be peculiar to ergot are common to other poisonous fungi.

Steering of Ocean-Steamers.—Among the reports of committees to the British Association at Dublin was one on the steering of screw-steamers. This report declares it to be an invariable rule that, during the interval in which a ship is stopping herself by

reversal of her screw, the rudder produces none of its usual effects to turn the ship. In fact, under these circumstances, the effect of the rudder is to turn the ship *in the opposite direction* from that in which she would turn if the screw were driving her ahead. A ship with screw reversed requires, in order to turn a circle, double the radius of that required while steaming ahead; and, if it is difficult to govern her direction, it is more difficult to predict what that direction will be. When moving at full speed a screw-steamer requires five lengths more or less in which to stop herself, whereas by using her rudder and steaming on at full speed ahead she would be able to turn herself through a quadrant without having advanced five lengths in her original direction. Moral: When collision is imminent, steam ahead and be quick with the rudder. But, owing to the imperfection of the steering-apparatus now generally employed, quickness is impossible, and it takes a long time to put a large angle on the rudder. "The result is" (so say the Committee) "that it is often one or two minutes after the order is heard by the men at the wheel before there is any large angle on the rudder, and of course, under these circumstances, it is absurd to talk of making use of the turning qualities of a ship in case of emergency. The power available to turn the rudder should be proportional to the tonnage of the vessel, and there is no mechanical reason why the rudder of the largest vessel should not be brought hard over in less than fifteen seconds from the time the order is given. Had those in charge of steamships efficient control over their rudders, it is probable that much less would be heard of the reversing of the engines in cases of imminent danger." Clearly this is a question which calls imperatively for regulation by the Admiralty or some other competent authority.

The Candle-Fish.—The eulachon, or candle-fish (*Thaleichthys pacificus*), an inhabitant of the Pacific Ocean in the vicinity of British Columbia and northward, is worthy of a place among the curiosities of the animal kingdom. It is a small fish—about fourteen inches in length—and in appearance resembles a smelt. It is the fattest of all known fishes; and, in fact, the Indians use it, in the dried state, as a candle. On touching the tail to the fire it burns with a bright flame till the whole is consumed; more usually, however, a wick of woody fiber is passed through the body of the fish from end to end to insure continuous combustion. But the candle-fish is also employed as an article of food, and in spite of its fatness—indeed, *on account* of its fatness—is highly esteemed by the Indians as a warming food for winter. For this purpose they are dried and smoked in the spring, and then packed away. So preserved they are eaten whole, or the oil is tried out and eaten as butter. The take is usually very large, and only a small portion is dried and smoked. The remainder are piled in heaps till partial decomposition has set in; they are then packed in large boxes and the oil pressed out. This oil also is used as food, and it is said to be not altogether intolerable to the stomach of civilized man. The appearance of the first shoal of candle-fish in March is greeted by the Indians with extravagant demonstrations of joy. It is their Easter.

The Storage and Purification of Water.—In one of a series of papers on "Water-Supply for Small Towns," now publishing in "The Plumber and Sanitary Engineer," Mr. E. S. Philbrick has some remarks on the best material for constructing cisterns. He gives the preference to brick, as being sufficiently durable, and at the same time cheap. As brickwork is not adapted to resist tensile strains, brick cisterns, if of any considerable size, can not withstand water-pressure, by the strength of walls alone. So we put them underground, getting the earth-pressure from without to balance the water-pressure from within, and at the same time protect them from frost. The circular form generally used is strong enough in itself to resist the earth-pressure when the tank is empty, for this is a compressive force. The extended application of hydraulic cement thus enables us to construct, in almost any part of the world where commercial relations exist, an imperishable and incorruptible water-tank, so far as its own materials go.

But cistern-water will always be more or less contaminated by the accumulation of dust swept down from the roof. Hence a cistern should be thoroughly cleaned from

time to time as occasion may require. Every cistern intended to hold water for drinking should have a filter. For this purpose a chamber can be parted off on one side the cistern, within which the suction-pipe takes the water for use, and the filtering material placed so as to pass the water through holes in this partition. Sometimes soft or half-burned bricks are used for this partition, through the pores of which the water passes freely, if sufficient surface be used. If, however, it be desired to remove any dissolved impurities from the water, these mechanical filters, whether brick, gravel, sand, or sponge, are useless, and recourse must be had to a charcoal or spongy iron filter, which acts chemically upon various substances in solution, burning them up, as it were, by the oxygen within the filter.

These filters, however, require such frequent cleansing for the renewal of their efficiency, that they can only be used effectively in a portable form, and cannot therefore be built into the tank.

Products of Coal-Gas Combustion.—From a lecture by Mr. Thomas Mills, before the British Association of Gas Managers, we take the following remarks on the products of combustion of coal-gas: "Of such gas, supposing its specific gravity to be ·5, or half that of air, a cubic foot contains about half its own weight of carbon; and, if this cubic foot of gas be burned, it will give a little more than half a cubic foot of carbonic acid, or, in weight, 488 grains. Again, a cubic foot of coal-gas, of ·5 specific gravity, contains about 41 grains of hydrogen, and this hydrogen in burning will produce 372 grains of water. If we regard the quantity of air necessary to supply the requisite quantity of oxygen to a cubic foot of gas, it lies between five and six feet of air. For every cubic foot of gas burned we require the oxygen of between five and six cubic feet of air, and this will give half a cubic foot of carbonic acid as a result. When we come to estimate the total products of gas-combustion in such a place as London, the figures are really startling. The quantity of gas consumed in London annually may be taken approximately at about 15,000,000 cubic feet. The amount of carbonic acid given off during a year by the combustion of these 15,000,000 of cubic feet is 433,000 tons. The amount of water produced by the combustion of this quantity of coal-gas is 360,000 tons, or 80,000,000 gallons. One of the largest, if not the largest gasholder-tanks in London, is at the Phœnix Works at Kennington. That gasholder-tank, supposing it had no internal cone, and were perfectly flat at the bottom, would hold 10,000,000 gallons of water, if filled to the brim. You might empty that tank and fill it eight times over in a year with the water produced by the burning of the coal-gas consumed in the metropolis during that time."

How the Penguin rears its Young.—In the southern part of the Indian Ocean—about latitude 40° south, longitude 80° east, or about half-way between Africa and Australia—are the two islands, St. Paul and Amsterdam, both of recent volcanic origin, and both the favorite resort of the albatross. But they are most of all remarkable for the number of penguins which have here their permanent residence. According to a writer in *Chambers's Journal*, these penguins form a rude sort of commonwealth among themselves. In the rearing of their young they exhibit considerable dependence on one another. The hens lay one or two eggs, never more, in a hollow of the ground or on a little grass. The task of incubation is performed by both parents, the one "off duty" going to the sea to procure food for itself, and when the young are hatched bringing a supply for the family. "Where tens of thousands of nests are collected together so closely that the visitor cannot walk without demolishing new-born nestlings or eggs at almost every step, it is difficult to understand how each bird knows its own nest, eggs, or nestling, as it appears to be the case until the young are able to walk about for themselves. Then the latter form into 'infant schools,' presided over by several matrons, and ask and receive food from any charitable passer-by, and the social system, so far as it goes, has attained its highest point. There is no longer any recognition of *meum* and *tuum*, but a determination on the part of each adult to do the best for the rising generation, without regard to the petty rights of property so

stoutly maintained and hotly contested in the egg stage. Woe betide the incautious or over-confident experimenter who shall remove one of these fierce motherly things from her nest with his hands!—the penalty will be a succession of stabs, which produce notoriously painful wounds. But the occupant of the nearest nest will always receive and tuck under her, together with her own brood, the young of a dispossessed neighbor. All through the nursery are well-beaten paths along which the birds hop in single file with most grotesque action to and from the sea; and from the nests on either side come sharp stabs at the legs of the intruder, a deafening roar accompanying his progress the while, and an odor assaulting his nose which only those who have sailed in a guano-ship can realize. The time has now arrived when the young must be taught their first swimming-lessons, and the rudiments of that aquatic life to which their special structure confines them. From the rookery to the sea they advance, hopping with both legs together, and jump feet foremost bolt upright from a ledge into the water. Then, and only then, are they thoroughly at home, and, making use of nothing but the powerful scaly flippers, dart about with the rapidity of a fish. Frequently the old bird will rise to the surface with a young one balanced on each flipper, maintained in its precarious position by the grasp of its own tiny paddles, and no doubt vastly enjoying this introduction to life and the novel experiences to be met with under water."

A Unique Surgical Operation. — A surgical operation of probably unique character is described in the *Lancet*, by Dr. Alexander Patterson—namely, the employment of a piece of dog's bone in the treatment of ununited fracture. The patient, while at sea, sustained a simple fracture of both bones of the left forearm. The arm was at once put in splints, and so remained for some weeks. On removing the splints it was found that the bones had not united. It was not till eight months after the occurrence of the accident that the man was admitted to the Western Infirmary of Glasgow. Repeated efforts were made to induce the broken bones to reunite, but all without avail; and finally it was decided to amputate the arm. In the absence of the regular surgeon, Dr. Patterson took charge of the case, and obtained permission to make an attempt at saving the limb. The operation is best described in the author's own words:

"The patient was taken and placed under the influence of chloroform, while at the same time a retriever dog was being anæsthetized. I made an incision along the ulnar side of the arm, cutting down upon the ends of the fractured bone, and removing the fibrous band which alone formed the bond of union; the rounded points were removed by the saw, and a hole drilled obliquely through each squared end. The same process was repeated on the radial side, when it was found that an interspace of about three-quarters of an inch existed between the two fragments of the radius. In the mean time, one of the senior students had exposed the humerus of the quadruped, completely denuded of every tissue except the periosteum. The length of bone was accurately measured (three-quarters of an inch), while from half an inch beyond the end of the necessary length the periosteal covering was rapidly but carefully dissected, the bone sawed through, a hole drilled in either end obliquely, as in the radius and ulna, and at once placed between the ends of the radius, where it fitted accurately. Wires having been passed through the holes, the bones were firmly tied together, the loose half-inch margin of the periosteum of the foreign bone being carefully spread over the periosteum of the radius. The wound was stitched with silver wire, the bone sutures coming out at each end of the incision. Wires were passed through the ulna, tied together, and the wound treated in a similar manner. The entire operation was conducted under the carbolic-acid spray. The arm was put up in gauze, and held in two rectangular splints."

We need not give details of the patient's condition from day to day. Suffice it to say that one wound remained open for twelve months, and that then the dog's bone, reduced to about half its size, came away, after which the wound healed completely. The radius seemed to have fallen in somewhat toward the ulna, leaving a slight deformity. The man is by occupation a ma-

rine engineer, and is now able to resume his ordinary pursuits. Dr. Patterson had hoped that the strange bone might find a new home for itself in the human arm. This failing, he was confident that it would secure perfect alignment and steadiness in the ulnar fragments. In the latter respect the event fully justified his anticipation. He still believes in the possibility of incorporating a foreign bone.

Ancient Hygiene.—It would be matter for a very interesting inquiry to ascertain how it happens that, with regard to many abstruse questions of practical science, hygiene for instance, the ancient Hebrews, Romans, Greeks, etc., reached results which for correctness put to shame the ignorance of later times. An illustration of this truth is given in a memoir by Dr. John Spear, lately published in the "Lancet." He first speaks of the precautions to be taken in selecting sites for human habitations. From the "Mishna" we learn how carefully all unclean things were removed from the vicinity of Jerusalem and the temple; and the investigations of Signor Perotti in the site of the ancient Jewish capital have shown how complete were the systems of sewers and the means of sewage precipitation and disposal. We find again that the Latin author Vitruvius, in his work "De Architectura," supposed to have been written in the reign of Augustus, in giving directions for securing healthy sites for towns, lays special stress on the necessity of a porous soil, and, in order to secure the ventilation of that soil, on perfect subsoil drainage. The views of Hippocrates on this subject, as also of Pliny and of other classic writers, might be studied at the present day with profit. Thus it would appear that the memorable researches of Pettenkofer, in a great measure, serve only to make us acquainted with the laws which were perfectly well known to the men of olden time. Then, as to practice: in the ancient cities of the world—Rome, Carthage, Herculaneum, Nineveh, and Alexandria, we know how well pollution of the soil was guarded against. What most judiciously executed works for this and other sanitary objects existed, recent discoveries have revealed. Probably in all these places too, and certainly in Rome, interment within the city walls was forbidden. "It is worthy of note," observes Dr. Spear, "that at this period of history pestilences and epidemics were not of common occurrence, and when they appeared they were usually clearly traceable to famine or to war. But to this enlightened and golden age succeeded one of darkness and intellectual torpor. Sanitary measures were forgotten or ignored; filth accumulated in crowded towns; the practice of intramural sepulture became general. The soil, the air, the water, we read, were impregnated with decomposing matters. As a result we have recorded those most destructive pestilences of the middle ages. The plague, the black-death, fever, and small-pox, swept over the land. . . . Pestilences were ascribed to the pleasure of Almighty God."

How to keep cool.—The experiences of an English visitor to the Paris World's Fair, as recorded in the "English Mechanic," convey a useful lesson on the means of enduring without serious discomfort the extreme of summer heat. This gentleman, Mr. D. Winstanley, writes that he went to Paris in March, the weather then being decidedly cold. As the temperature gradually increased he noticed that his ordinary clothing became uncomfortable whenever at 8 A. M. the thermometer indicated 70° Fahr. Accordingly, when that temperature was indicated, he made it a rule to adopt linen clothing, and he then enjoyed a comfortable temperature throughout the day. As summer advanced and the heat increased he never felt hot when clad in linen. Even when the thermometer had risen to 97° Fahr. in the shade, he felt no uncomfortable sensation of being hot, and, furnished with a "havelock," strolled leisurely in the blazing sun for hours, the thermometer indicating 125° Fahr., without discomfort, and without consciousness of perspiration. Mr. Winstanley adds, however, that during the hot weather he lived almost wholly on vegetables and fruits—peas, beans, melons, etc. —using no meat, and above all no fat. He takes occasion to commend the French style of windows in dwelling-houses. "Instead of our miserable idea of an horizontal section," he writes, "which permits at most only one-half of the window aperture to be

opened for the admission of air, the French employ the vertical division, and open the windows inwardly on hinges as we open doors. The window apertures are also large, or rather high, descending to within eighteen inches of the floor, and ascending to within four inches of the ceiling. Outside these are latticed shutters. When the windows are open and the shutters closed the sun is effectually kept out, and the free circulation of the air is scarcely interfered with. A light balustrade of iron, within the shutters and without the glass, serves to prevent falling through the windows."

Destruction of American Forests.—It has been for some years apparent that the United States supply of timber must fail at no distant day, unless some concerted measures are taken for growing new forests or in some way preserving the old. The present condition of the lumbering business will be understood from the following facts, published by the St. Paul "Pioneer Press," and based on the observations of Mr. James Little, a lumber-merchant of Montreal, who has long studied this subject: Of the twenty-six States comprising the New England, Middle, Western, and Northwestern, to the Rocky Mountains, only four are now able to furnish lumber-supplies beyond their own requirements; the four being Maine, Michigan, Minnesota, and Wisconsin. But Maine is almost stripped of her pine-forests, and lumberers have to go to the head-waters of the rivers in search of spruce, while mere saplings, six or seven inches in diameter, go to the mill. In a few years Maine will have neither pine nor spruce for home consumption. The northern parts of Michigan, Wisconsin, and Minnesota, are the only localities of the whole twenty-six States which can furnish supplies of white pine beyond the home demand; but they will not be able to do so, Mr. Little affirms, for more than five or six years longer. The main streams are all stripped, and the dependence of the lumbermen is now on the head-waters of the tributaries. In 1870, according to the census report, there were in the United States 173,450 industrial establishments, employing 1,093,202 hands, devoted to the manufacture of wooden articles. The impression prevails that when our supply of lumber fails, as it must inevitably within the next ten years, we can find in Canada a supply that will not be exhausted in centuries. But this is an error; at least, Mr. Little asserts that there is not from Manitoba to the Gulf of St. Lawrence as much pine, spruce, hemlock, white-wood, and other commercial timber, as would supply the United States for even three years! In the light of such facts as these, it behooves the people of this country to seriously consider the subject of reforestation and the protection of young timber-trees.

Movement of Water in the Suez Canal.—The currents of the Suez Canal and the action of the prevalent winds on the water therein have been studied by M. Lemasson, who finds that Lake Timsah and the basin of the Bitter Lakes, the former in the middle of the line of navigation, the latter nearly at the middle of the southern branch of the canal, constitute two great regulators, at which the tidal currents from the two seas respectively expire. The north and south branches of the canal are not, however, independent as regards the movement of their waters. The dominant winds in this region blow, from May to October, from the north and northwest, and raise the mean level of the waters of the Mediterranean at Port Saïd, while they depress the mean level at Suez. The difference of level, which attains almost sixteen inches in September, sets up, in summer, a current from the Mediterranean to the Red Sea, which is interrupted by the tides, but nevertheless carries a considerable volume of water from north to south. In the winter, on the contrary, the south winds blow strongly, and the mean level of the Red Sea is then higher than that of the Mediterranean, the difference attaining a maximum of nearly one foot. The general direction of the current of the canal then sets from the Red Sea to the Mediterranean. The volume of water flowing annually from sea to sea is estimated at 400,000,000 cubic metres, and this with the tidal currents annihilates the effects of evaporation at the surface of the lakes, and aids the solution of the great salt deposit in the basin of the Bitter Lakes, which, instead of increasing, is diminishing, especially in the line of transit of ships.

The Parasites in Pork.—*Cysticercus cellulosæ*, familiarly known as "measles" in pork, on passing into the human economy may develop into the common tapeworm. Analogous entozoa are sometimes found in beef, veal, mutton, and other meats, but the great source of these formidable parasites is pork. They can be easily detected in the carcass of a pig that is infested by them. "They are in the muscles," writes Dr. Vacher, in a paper read before an association of health officers, "between the fibers, between the muscles, on the surface of the muscles, and even in the walls of the heart. . . . Specimens from the same beast," he continues, "are nearly of the same size, but specimens taken from different beasts vary considerably in size. The egg-shaped investing bladder is scarcely ever less than an eighth of an inch in length, and it sometimes measures half an inch, so that it may be easily seen and removed. It is semi-transparent, and contains a clear fluid, and what looks like a little white ball. On transferring the bladder to a glass slip, a touch with the point of a knife will suffice to rupture it, and if you then press a cover down upon it you have a preparation in which the *rostellum* and circlet of hooks may be distinctly seen with an ordinary lens." When meat is a little dry from exposure to air the cysts collapse, and are not distinctly visible. Dr. Thudicum recommends that such pork be submerged in water which the cysts will absorb by endosmosis.

Proposed Domestication of the African Elephant.—While the Asiatic elephant is in India domesticated and employed as a beast of burden, the African elephant, living, has no economic use, and is merely hunted for "sport," or for its tusks, hide, and flesh. It is now proposed to attempt the utilization of the African elephant as an aid in the exploration of the "Dark Continent" and for the transportation of goods from the coast to the fertile plateaus of the interior. Even in the Cape Colony, to say nothing of central Africa, elephants are numerous, and one troop has been observed within fifty miles of Port Elizabeth. Sir J. Fayrer suggests that on this troop the attempt at domestication might first be made. According to him, the African elephant is as well fitted for labor as the Asiatic, and could be as easily tamed and trained. That this is the case is amply proved by the state of docility to which the male and female African elephants in the London Zoölogical Garden have been reduced by their keeper. They are just as obedient, intelligent, and free from vice as their Asiatic congeners, and there appears to be no room for doubt that they might be utilized to just as good purpose. The importation of a few of the officers who have had experience in catching and training elephants in India, together with a few trained Indian elephants to commence the work, would very soon put the value of the project to the test.

Relation of Brain-bulk to Intelligence.—From observations made on numerous series of human crania, Dr. Lebon, of Paris, infers that intelligence is in proportion to the volume of the cranium. By comparing these series of crania, it is also found that the superior races present a much greater number of voluminous crania than the others. The same phenomenon is presented in proportion to the degree of civilization; the Parisian crania of the twelfth century present, for example, a less volume than the crania of modern Parisians; at the same time the difference between individuals becomes more considerable. Dr. Lebon does not believe that stature exercises any considerable influence on the volume of the cranium and the weight of the brain. Nevertheless, with equal height, the woman has a brain less heavy than the man. The author, from a study of seventeen male and seventeen female brains, found between them a difference of 172 grammes to the advantage of the former. It is worthy of remark that, among the superior races, the cranium of the women is generally much less than among the inferior races. This is due, Dr. Lebon says, to the insignificant part taken by woman in the work of modern society. There is a constant inequality of development between the two halves of the brain, which is sometimes more developed on the right, sometimes on the left, without race or state of intelligence appearing to have any manifest influence on the direction of this inequality of development.

NOTES.

In the death of George Henry Lewes, which occurred in London, December 1st, the more serious literature of England lost one of its best representatives. Mr. Lewes possessed a very remarkable degree of native intellectual power, and this gift of nature he appears to have turned to the best account by stern self-discipline and assiduous and well-regulated study. He was a worker in many different fields of literary activity, in some of which he proved himself a master, while in none did he sink to mediocrity. His learning was profound and accurate, and his control of his intellectual resources was complete. In our ninth volume we published a brief biography of Mr. Lewes, with portrait. He has since that period been engaged on the "Problems of Life and Mind," but, as far as we know, has not published any volume since the second.

An Englishman traveling in the Rocky Mountains, in company with an American astronomer, severely criticised the hideous defacement of picturesque places by big-lettered advertisements on every conspicuous rock. His fellow traveler readily admitted that the custom admits of no justification, but added, "I guess we are not as bad as some of your people who have tried to advertise themselves on the planet Mars" (by naming Martial objects after themselves or their friends).

A new illuminating gas is now, according to the Liverpool "Post," under examination by the British Lighthouse Board (Trinity House). It is declared to be not only cheaper than ordinary gas, but far more effective. It is capable of so much concentration that the quantity contained in a small buoy has supplied a light burning for twenty-eight days with sufficient brilliancy to show the position of the buoy to passing ships.

Dr. Lyon Playfair complains that, whereas the 30,000 medical men of England are protected by law against the competition of unqualified practitioners, the 150,000 teachers have no such protection, and have no recognition in the face of the law as constituting a distinct profession. Any one who has failed in any other calling may, if he please, open a school or seminary, and no one can question his right. Dr. Playfair intends to move in Parliament for a bill to determine the qualifications of teachers, and to exclude from that profession all but duly registered aspirants.

The gas-wells of East Liverpool, Ohio, are worthy of being ranked among the "wonders of the world." They are situated, writes a correspondent of the Cleveland "Herald," in and around the town, and give it a continual supply of light, the gas being almost as free as air. It costs practically nothing, and both heats and lights the town. The street lamps are ablaze day as well as night, for it costs nothing to supply the gas, and it takes trouble to shut it off. Then the gas is almost the only fuel employed in the town, being conducted into the grates and stoves by pipes. It is also used for generating steam-power for sundry great pottery manufactories, employing upward of 2,000 workmen. The first of the wells was opened twenty years ago, and there are no signs of exhaustion.

In the discussion of the profits of wheat-growing to-day as compared with the profits forty or fifty years ago—a discussion suggested by a strike of agricultural laborers in an English county—the very curious fact was brought out that the wheat itself represents only one half of the produce, the other half being represented by the straw. The price of wheat has been declining for years, but the price of straw has been rising, till now it is actually the more important, in England, that is to say. The demand for straw as litter, for fodder to mix with other foods, and for different branches of manufacturing industry, has so increased that the article is scarce even in rural districts.

A writer in "Land and Water," under the signature of "Bangkok," gives this amusing account of a little scene which he witnessed at the court of the "Second King" of Siam: "After sitting down and lighting our cigarettes he (the Second King) rang a little hand-bell, and in the dimly-lighted veranda I saw three figures wriggling along on their stomachs to his majesty's chair, on reaching which they sat up on their haunches and showed themselves, three handsome white-haired old men who were introduced to me as the royal astronomers." They had been summoned for the purpose of exhibiting to the foreigner their calculations of an approaching solar eclipse, which proved to be very nearly exact. After a little present from their master, the old gentlemen retired as they had come.

The "Holy Synod" of Athens having ordered public prayer for rain to be made in all the churches of its jurisdiction, one priest, according to the "Independent," made the following common-sense remarks to his congregation: "Blessed Christians," said he, "our most holy synod has ordered public prayer to be made for rain. But I have been considering that, although we in Attica do indeed need rain, in Peloponnesus it would be fatal to the olives and currants. Therefore, blessed Christians, I leave the matter to each one of you. As for me, I am quite willing to leave it to God to do as he chooses."

CHRISTIAN GOTTFRIED EHRENBERG.